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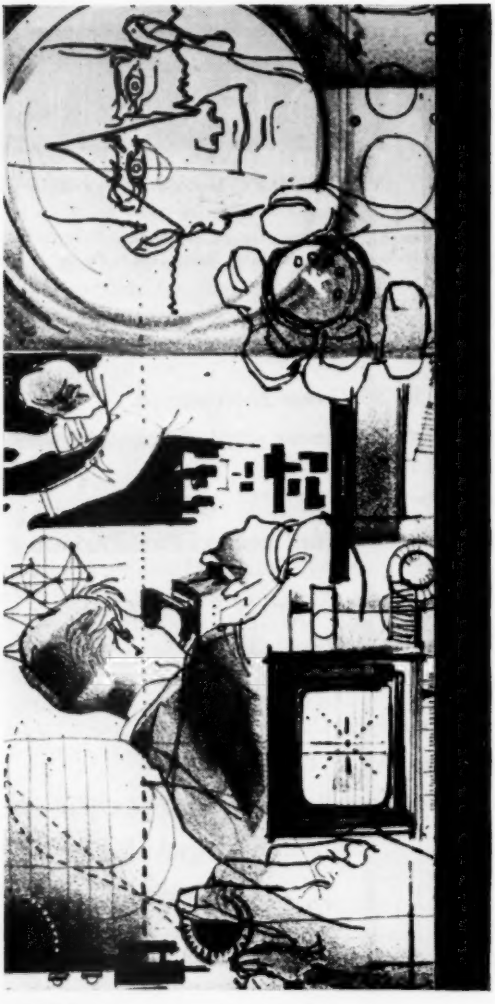

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Notes and Comments

World Mental Health Year

The World Federation for Mental Health is the international non-governmental organisation accredited to United Nations which integrates some 120 voluntary professional and mental hygiene associations in 46 countries in their efforts to rouse their governments and public opinion to tackle the problems posed by psychiatric illness and handicap.

In many instances this task of public education involves the change from demonology to natural science in countries now undergoing rapid technological change and political emancipation. Traditional ways of coping with the perennial problems of mental sickness and deviation, and of regulating human relations and social behaviour are being swept away. Vast populations are displaced, not only in the sense of being political refugees, but also by social mobility, rapid urbanisation and the loss of 'safe roots'. Neurotic insecurity and delinquency are by-products of such upheavals.

It is this process in many countries which determined the Executive Board of the World Federation to launch an effort to capture the attention of universal public opinion, by analogy with the highly successful Geophysical Year. The latter proved that scientific co-operation in a major enterprise can transcend parochial boundaries and metallic curtains.

To be sure, the World Federation and its many national co-operating Mental Health societies are alive to the difference between what is possible in the physical sciences with their 300-year lead and their resources, and the limitations and even public resistances to the new and poorly-found behavioural sciences which often challenge political dogma; neuro-physiology, psychology, psychiatry, anthropology, sociology—and their applied skills expressed in social casework, psychotherapy, educational psychology, industrial relations techniques, community planning and so forth. Indeed, this abridged list of the disciplines represented in the mental health movement indicates the broad scope of W.H.M.Y. We may well ask whether these young branches of knowledge have as yet the background of learning to appear before the world as a united body seeking to persuade people in power to adopt these as the basis for policy making and human betterment.

It is true that propositions and hypotheses in the fields of human personality and of group behaviour are still tentative and often conflicting, and that research methods for validating them are seldom of a level of sophistication and rigour demanded by the physical sciences. But it is precisely this point which has acted as a spur to the promoters of World Mental Health Year. It is the requirement of these disciplines to become more reliable! How could research facilities and means be provided but for an 'infra-structure' of governmental support, in turn based on public recognition of the need?

The study of infectious and epidemic diseases was in this position only a short while ago. Now public health work has time to turn to mental disorder. In many countries not even the elementary institutions for the care of the mentally sick exist, let alone any facilities for training of personnel, medical and ancillary, in current practice or in systematic data collection and study. Even in Britain, though nearly half of our hospital beds are occupied by the mentally ill or subnormal, and though neurotic disability is second only to respiratory infections as the cause of sickness-absenteeism, only some 5 per cent of research monies are devoted to investigation of the complex causal factors or of the rational prevention and

alleviation of these conditions, which seem rooted in the very fabric of culture and human relations.

If we add to these obvious disorders the more endemic psycho-social scourges of mankind, for which as yet not even a medical terminology exists; such as political and religious fanaticism, ethnic prejudice, aggressive nationalism, anti-social delinquency and violence—all of them proven killers on a scale that dwarfs cholera, typhoid or tuberculosis—then it does seem that the organisers of W.M.H.Y. are indeed to be congratulated on seeking to show to a world public what is involved and what is at stake. Is this not perhaps even a greater priority for co-operative effort in scientific and technical discovery than the exploration of outer space? Enough is known to come forward with at least one proposition: that human nature and its aberrations are a rewarding field of study, which may yield the greatest fruits. W.M.H.Y. is the affirmation of this faith that by 'the proper study of mankind' we shall gain a degree of control over the 'inner world' of human motivation and its vagaries which can stand comparison with the physical sciences, and become in this sense masters of our fate rather than remain as hitherto victims of the irrational.

H. V. DICKS

The head of a desert locust. (*F.A.O. photo by Jean Manuel.*)

Microscopy

A recent report by D.S.I.R. indicates that during 1959 it increased the number of grants to universities and technical colleges for researches in every field of science except mathematics. The report notes that workers at the University of Cambridge have achieved a break-through of great significance in the field of metallurgical research, using electron-transmission microscopy. It is now possible to follow by ciné-techniques the arrangement and motion of dislocations when a metal foil is strained to breaking point. Microstructures virtually down to the atomic scale can be seen for the first time. It can also be observed, for instance, what happens when a pure metal is suddenly cooled from a high temperature, and how nuclear radiation damages the crystal structure. This new technique of transmitting through a thin metal foil offers a great opportunity to understand thoroughly how the engineering properties of solids result from atomic structure and properties.

The Locust

The Food and Agriculture Organisation in Rome reports the signing of an agreement recently between the United Nations Special Fund authorities and representatives of fourteen countries as a result of which something like £1,400,000 will be spent during the next six years on a campaign to control the locust.

Dr. Taskir Ahmad, the Chairman of a special F.A.O. desert locust meeting, noted at the meeting that the locust 'is probably the longest migrating insect on earth. It can travel up to 100 miles a day and can go on for six or seven days—that is six or seven hundred miles practically without eating. It has a wide range of tolerance to temperatures and at 115° F. feels quite happy and at home when other insects are beginning to expire from the heat. It can also withstand the low night temperatures that come in the desert. In fact, the

Locust invasion, French Morocco, locusts squashed by the cars are immediately preyed on by the survivors. *(Photo by Studios du Souissi, Rabat.)*

desert locust enjoys the heat and dryness of the atmosphere of the desert, and needs the moisture to be found in the soil to hatch its eggs. It has fewer enemies than any other creature—almost none of any importance as far as we know. It also has an extremely interesting life-cycle consisting of two phases. In the first, it lives a solitary existence. Its hoppers are yellow or greenish yellow in colour which gives them excellent camouflage but on multiplication they change, becoming gregarious and taking on a bold yellow and black colour. As a young insect it is sedentary by nature, lazy and wants to hide. As soon as it gets together with other locusts, it becomes gregarious, bold and very active. These characteristics add to the difficulty of combating locusts because it is difficult to discover them in their solitary phase and there is little time to deal with them once they start to swarm and migrate.'

'We were interested to find out what made the solitary green hoppers of locusts turn black when they met other hoppers,' he said. 'In one experiment we put two green hoppers in a glass and the effect socially was that they both became black. When we separated them they reverted to their original green. As a further test we put a hopper in a space surrounded by mirrors and the sight of himself caused him to jump excitedly and eventually change colour. It became obvious that the black pigment was due to muscular activity, a conclusion which was further supported by an experiment in keeping the hopper on the move by artificial means, as a result of which the creature turned black.'

The Council for Nature

Although the Nature Conservancy has been in existence for eleven years popular interest in the

conservation of nature has only recently become appreciable. The strength of the movement for nature conservation and the study of natural history has become apparent in the formation of the Council for Nature, the success of the Conservation Corps organised by the Council for Nature, and the growth of county naturalists' trusts. In its brief life of two years the Council for Nature has achieved the support of 230 natural history societies, both national and local, has been concerned with the dangers to wild life of toxic chemicals and given evidence at some half-dozen public enquiries where development has threatened sites of particular scientific interest.

The Council's Conservation Corps is now in its second year and by the summer of 1960 some 600 people, mainly young, will have worked on sites of natural history interest ranging from Box Hill in Surrey, to Ben Eighe in Ross-shire. The work undertaken has included scrub clearance, path making, pond cleaning, fencing and the removing of dangerous dead wood from areas liable to a high fire risk. The beneficiaries are local naturalists' trusts, the National Trust and the Nature Conservancy. The latter meets the cost of parties working on its reserves, but the majority of the expenses for the other work is covered by a grant from the Carnegie United Kingdom Trust. The problems of conservation on a particular site are explained before work commences and, assisted by talks and visits by local naturalists, the volunteers have an excellent opportunity of studying nature throughout their stay.

H.R.H. The Duke of Edinburgh has been patron of the Council for Nature since its foundation. On 8th July he visited the Conservation Corps at Wicken Fen; the Nature Conservancy reserve at Orfordness; Havergate Island, the breeding place of avocets, maintained by the Royal Society for the Protection of Birds; and Flatford Mill, a centre of the Field Studies Council. This royal tour of sites of natural history interest gave great pleasure to all lovers of the countryside and particularly to naturalists who have over the last two years worked so hard to establish the Council for Nature as an effective central organisation.

D. J. B. COPP

Films for the A.A.A.S. Annual Meeting

As in previous years the 127th Meeting of the American Association for the Advancement of Science will include an international programme of scientific films. The meeting will take place in New York from December 26th to 31st, and those wishing to suggest suitable 16 mm research or documentary films should communicate with Raymond L. Taylor, Associate Administrative Secretary, the American Association for the Advancement of Science, 1515 Massachusetts Avenue, N.W., Washington 5, D.C., U.S.A.

THE TWO ASPECTS OF SCIENCE*

By Sir GEORGE THOMSON, F.R.S. *The Master, Corpus Christi College, Cambridge*

Science is already valued for what it can do to increase man's control over nature, and feared for what some of its consequences may be. Some would have us consider these consequences as the sole justification of science. This view, or something very like it, is the official attitude in Marxist countries, and there are many in this country and in the U.S.A. who would be horrified to be told they were Marxists but who without any explicit statement do in fact act and speak as if they thought the same. But this view is too limited, as I hope to persuade you in this Address. There is a second aspect, it is this . . . . Science aims at understanding the nature of things; in this it is at one with religion and philosophy. But its approach is the opposite. These last try to gain knowledge of the whole, in the one case by an awareness of the deity, intuitive or revealed, in the other by building with words a system of thought which can account for fundamentals. Science starts from the other end. It begins by studying details, often apparently trivial details but things which are queer and appeal to human curiosity. Things like black rocks which attract iron or rubbed amber which makes chaff fly.

This has been more successful than one could have expected. It is a method difficult to defend *a priori*—it has often been made fun of. Swift ridiculed the philosophers of Laputa who studied how to make sunshine from cucumbers.

But the method works.

From the study of details such as these come *concepts*, these concepts, or some of them, show vitality and take charge, as characters in fiction are said by authors to do.

They are the bases for hypotheses and 'laws', they are the things that get names—mass, energy, temperature, entropy, wavelength, charge, electrons, quantum numbers, to take some examples from my own subject.

Some do not survive: caloric, phlogiston. Others are deposed from their independence and become vassals of the more successful, as mass has of energy or valency of electronic structure.

But the successful concepts—or the best of them—are not concerned with details any more, they penetrate deep into the heart of things. Electrons for example are regarded as present in all ordinary matter, and in any attempt to explain the behaviour of matter, physical, chemical or ultimately biological one is, if one goes deep enough, forced back on to them. At present they stand as a fundamental concept, but even if, as is quite possible, they ultimately come under some still more general idea, the concept of an electron will still be used, as mass is in mechanics.

It unifies our thoughts over a vast area of facts. Thus there is excellent reason to believe that the whole of chemistry is explicable in terms of electrons and the wave functions which describe their location. This is an enormous simplification of thought, even if the mathematics are too difficult to work out in most cases. It does not much matter from this point of view that test tubes are cheaper than electronic computers if you really want to know the answer to a practical question.

Scientific concepts enable certain aspects of the enormous complexity of the world to be handled by men's minds. They are suggested mostly by experiment but partly by mathematics, and controlled by the need that they should not lead to illogical consequences.

These concepts represent an extension of the human intelligence. They are not easy, many have subtleties which, for example, oblige the populariser to take anxious care lest in trying to simplify he make statements which are simply untrue. Some

* Presidential Address delivered to the British Association at Cardiff on August 31, 1960.

are more fundamental than others, but even those which turn out to be only rough pictures of what really happens often retain their usefulness. They are sketches as compared with finished pictures, and if details are not needed a sketch is often clearer.

Concepts are discoveries as well as, indeed more than, inventions. They have more in common with the discovery of America than with the invention of the spinning jenny or of the electronic computer. Some at least have been forced on unwilling minds by hard facts and have resisted many attempts to displace them. I am thinking more especially of the quantum theory.

Not merely are they an exercise of the human mind which equals the brilliance of any system of thought, philosophical or even mathematical, but they represent reality.

The nature of the relationship is to me a mystery, but they are certainly not merely the product of the human mind.

Science is a pyramid based on many varied facts, topped with a crown of ideas reaching to the skies. These are its fruit but can change without affecting its stability. I sometimes think that in philosophy the pyramid is the other way up!

What we want as scientists—I am sure in this I speak for the great majority—is that the world should realise that we are not interested merely in making possible new drugs, television sets, or weapons, though all these are important, but in enlarging the bounds of human knowledge.

The greatness of the human race is indeed many-sided. Thus in the world of art there is a difference between the ability to compose or interpret a great piece of music and that which writes a great novel or paints a great picture. Yet all are evidence of greatness and worthy of the name. Still more so is moral greatness. 'There is one glory of the sun and another glory of the moon and another glory of the stars.'

I have no desire to exalt our profession unduly, but surely the ability to understand, even to a limited extent, the world around him is one of the powers of which man has good right to be proud. Indeed it seems to be the one which most divides him from the animals. The lark's song, the heroism of a plover luring an enemy away from her young, the cat who was seen to attack a grizzly in defence of her kittens—these, if taken at their face value, are notable even by human standards, but I am not aware (though I speak with great diffidence) of any creature which even appears to be trying to discover any general principles.

The chance of understanding things of fundamental and permanent importance is what makes the pursuit of science fascinating and worth while.

These two aspects of science do not in fact conflict. The best way to make advances in technology, whether on the medical or the engineering side, turns out to be to understand the principles. This is quite a recent discovery, indeed it has probably only recently become true. It would not have been much use for example to man in the stone age, or even a few hundred years ago, to try to understand the principles of tanning with no basic knowledge of chemistry to guide him. He did better by trial and error. Even the steam engine was developed with little knowledge of the determining principles, though the best scientific minds of the day were much interested and the thought they gave it advanced science by discovering thermodynamics.

Electrical engineering was the first important activity to be developed from the start on scientific principles, and even here Edison did great things on a decidedly sketchy knowledge. But increasingly more discoveries are made in research laboratories and fewer by workmen on the bench. This is what one might expect. As fundamental principles get known it is possible to use their consequences intelligently. Often these consequences are too complicated to be calculated, but knowledge of the principles is an enormous help by showing in a general way what is likely to happen.

Then of course there are the modern technologies which derive directly from some important new basic discovery, such as electronics and nuclear energy.

This dependence of technology on pure science is now pretty generally recognised by industry. The more progressive industries maintain research laboratories which both make use of the basic discoveries made in the universities and elsewhere, and contribute their own. These laboratories may be owned and operated by individual firms, or be co-operative efforts of an industry in the form of Research Associations substantially helped by Government. There are black spots, of course, industries which spend too little on research or

organise it badly, but broadly speaking the need is realised and reasonably well met.

Pure science receives a great return for what it contributes to technology, and this in two ways, materially and in the realm of ideas. Consider first the material return; a modern physics laboratory could not work without instruments developed for technology and obtainable cheaply because industry needs them in large numbers. Take just two examples out of many. The use of high vacua—and how important this is—has been enormously facilitated by the needs first of the electric light industry and then of the manufacturers of radio valves. Again, the complicated electronic devices which crowd every physics research laboratory would be impossible without the cheap components of all kinds manufactured originally for radio and now for television. For this return by industry I sometimes think we academic scientists are not as grateful as we ought to be.

The other influence of technology is more subtle but as important. As science advances, concepts tend to become more and more abstract, further from anything that can literally be touched or handled. To take a simple case, energy is more abstract than mass which it replaces. In the higher flights of theoretical physics abstraction goes much further. Is there not a danger that one may lose touch with reality, and end up by supposing that some elaborate piece of mathematics represents reality when it is only a creation of the mind, inspired indeed by physical reality but no more like it than is a modern picture? I think we are safe as long as the people who make these theories are reasonably close to them who use them, not merely in laboratories, but in industry.

A theory which involves detailed consideration of the behaviour of particles less than a millionth of a millionth of an inch across would have seemed to Swift too absurd to be even worth ridicule. Yet one cannot regard it as a pipe dream when it leads to the great reactors of Calder Hall or Chapelcross. To me, the most amazing thing about science, and the most surprising and exciting fact about our world, is this astonishing connection between highly abstruse theoretical ideas and the matter of fact, in this case the housewife boiling her kettle with power from nuclei. This surely adds an immense thrill to discovery. I have no sympathy with those who regard technology as intellectually a poor relation of science. To me science without technology is incomplete and inconclusive. Systems of philosophy come and go, some are perhaps true but who can tell? But when conclusions deduced from precise experiments by mathematical theory lead to detailed predictions from which working machines can be designed, machines which without the theory no one would have thought of in a million years, then indeed one knows that one lives in a universe which is rational and that one has found the key to one of its rooms.

In speaking to you, as I am tonight, to stress the importance of the idealistic—almost the spiritual—side of science, I am hoping that my words may reach others not here who are brought less closely into contact with it than are most of you.

May I remind you of a few of the ways in which scientific knowledge has influenced and is influencing thought and the intellectual climate of the world? First and most obvious is the idea of evolution, that things change consistently and pass through successive stages, whether they are mice or stars. That living creatures in particular not only go through individual changes, but that in the flow of generations changes come over the race, or over a part of it, and that these changes lead to profound differences like those which distinguish men from molluscs. In the year of the centenary of the *Origin of Species*, just past, you have heard the profound consequences of this idea described much better than I can.

In the course of my life the quantum theory has produced a revolution in physics comparable with that produced in biology by Darwin. We have been forced, some of us very unwillingly, to believe that at bottom the laws of physics are not statements of what *must* happen, but of the relative chances of a variety of alternatives. That the determinism of the planets, for example, occurs merely because planets are enormous on the atomic scale and their distance from the sun very large, and that the *Nautical Almanack* would be impossible if the solar system was reduced to the scale of a molecule. Certainty comes with a massive body, or if the bodies are small one must have very many of them so that the laws of statistics can manufacture near certainty out of highly uncertain events, as they do in life insurance.

This makes quite a difference to the way one regards the world and I think its consequences are

still not realised by the average educated man, though it has been accepted by the majority of physicists for thirty years. It shows for one thing how dangerous it is to extrapolate, to attribute—in this case—to the very small the kind of behaviour that is common sense when one talks of objects of large or ordinary size. That for example if a particle crosses a flat screen with two holes in it, it must have gone through one to the exclusion of the other. This is not true of an electron.

The quantum theory stresses another point which is fundamental to the modern view of physics—the importance of the observer. An experiment to be any use must be observed. True, the immediate observer may be a photographic plate which forms a latent image to be developed and examined later, but there must be something. In other words, the scientist can only learn about the world through his senses. Theories are meant to unify sensations and in this sense explain them, but on these sensations they ultimately rest.

The observer first received proper attention in the theory of relativity. As long as there was supposed to be an aether there was a privileged observer, or class of observers, those at rest with respect to it. Without it all are on the same footing, and equally entitled to be considered. Now relativity asserts that the laws can be stated so as to be the same in form for all, no privilege is allowed.

But relativistic observers, those rather strange creatures who go about with clocks and measuring rods making signals, are not supposed to alter what they observe. The quantum observer does. Or more precisely, the circumstances that attend observation, for example the light that is required, alter what is observed and in a manner which cannot be determined even after the observation has been made. In consequence the knowledge that can be acquired is limited, because each observation upsets something else one would like to know. This was the first real hint that there are definite limits to scientific knowledge, limits not dependent on human patience or ingenuity. The argument depends, as all physicists know, on the finite size of Planck's quantum of action, but a very similar conclusion might have been reached before the quantum theory was thought of.

I think it is curious that physics remained deterministic in philosophy as long as it did. The power of accurate prediction in all systems diminishes with time. Even for the planets, those most orderly of creatures, a Nautical Almanack drawn up for the year 10,000 would be appreciably less accurate than one for next year. When one is interested in individual atoms this loss of accuracy can be enormous and catastrophic. Quite apart from any quantum considerations, even if one knew the positions and velocity of every molecule of a small sample of gas at one instant one could not predict the path of a special molecule distinguished, for example, by being radioactive; one could not find, let us say, where it would first hit a wall of the vessel holding the gas. Even supposing every gaseous collision fully determinate a small error in an original measurement mounts up, as Max Born has pointed out, with enormous speed. In a small fraction of a second all the detailed information is useless, and one can only treat the motion as one of diffusion to which only a probability answer is possible. So when time is taken into account one gets much the same result even if Planck's constant were many orders of magnitude less than it is. This kind of virtual indeterminacy is inherent in Maxwell's kinetic theory. It sets as real a limit to certain kinds of human knowledge, even given any thinkable extension of human skill, as does the quantum theory. The circumstances to which it applies are almost certainly of more human importance. The further ahead a prediction is made the less certain it becomes, and beyond a certain rough limit all that can be affirmed is a probability, often over a wide range of possibilities. It is an interesting question, for example, what is the best that could ever possibly be done in the long range forecasting of weather. Can, say, a prediction of the weather on a particular day ten years hence ever have any validity?

Yet in some respects atoms behave in a surprisingly straightforward way. They pile together almost like spheres, like tennis balls in fact. They are slightly compressible and their distances depend a bit on chemical relationship, but one can place them in order in molecules and still more definitely in crystals. The work which is being done by the group at Cambridge under Perutz and Kendrew is a striking example of this. They have located most of the 10,000 atoms in the vastly complicated molecule of haemoglobin, the first protein for which this has been done.

Single rows of atoms, and occasionally indi-

vidual atoms, can actually be seen in the electron microscope. They seem as real, and almost as commonplace as grains of sand.

In contrast to this matter-of-fact behaviour of atoms the electrons are strange fairylike creatures. They have no particular place, even when part of an atom one can at best assign them a region in which they are most likely to be found. Unless they have an unusual amount of energy they cannot be assigned a definite path. Even their approximate behaviour is queer and to treat them properly requires highly abstract mathematics.

They can be created out of the energy of radiation but only in pairs with positive and negative charges, and the positive one soon dies in a suicide pact with another negative. In the same way protons and antiprotons can be created and disappear. Certainly other kinds of elementary particles also have their anti-particles.

I should like to conclude by referring to two ideas of a somewhat more speculative character which may prove of importance in general thought.

The first is the relation between mass, energy and matter. In conventional mechanics mass is the primary property of matter, even more so perhaps than extension since it is more constant. Now, as you know, the theory of relativity made it probable that mass is energy considered from another point of view. This is expressed by Einstein's famous equation $E = mc^2$. Thus if energy is added to, or taken from, an otherwise closed system the mass of the system will increase or decrease, as the case may be, though it takes a lot of energy to make much difference to the mass. Einstein's conclusion has been abundantly confirmed by the discovery of nuclear energy and other experiments in nuclear physics. Further, as I have just said, energy can be transformed into pairs of particles of opposite sign. It began to look as if matter is just another name for energy.

It may seem paradoxical to equate energy with the property, mass, that measures inertia, but remember that in Newtonian mechanics the bullet penetrates because of its inertia which makes it continue in its state of motion. However, mass is only one property of matter and perhaps not even the most important. There are indications, rather slight perhaps, but not to be ignored, that matter is *not* just another name for energy. The study of the many curious particles which have been found is cosmic rays and later produced in the giant so-called 'atom-smashers' has shown the persistence of certain features in spite of the bewildering number of spontaneous changes which these particles undergo. Two groups of these particles have appeared such that the net number in each group remains always the same, and this in contrast to a third group for which there is no such constancy. By the phrase 'net number' is meant the difference, in each case, between the numbers of the 'ordinary' particles and of the antiparticles. Thus electrons are a member of one group called 'leptons', to which neutrinos also belong. In reckoning the net number of electrons one subtracts the number of positrons from the number of ordinary electrons. Thus the creation of an electron-positron pair does not alter the net number. The rule states that no interaction between particles of any kind, including the photons of radiant energy and the mesons of the cosmic rays, can alter the *sum* of the *net numbers* of the three kinds of particles, electrons, neutrinos and μ mesons, which count as leptons. A similar rule holds for the class of particles, protons, neutrons, and some others, which rank as 'baryons'. Leptons can never change into baryons or reversely.

This seems a hint that underlying matter are two classes of entity, each of which indeed can appear in several different forms associated with various amounts of mass, sometimes electrically charged and sometimes not, but yet fundamentally the same. Matter is I think more than merely mass or energy. A neutrino has zero rest mass but yet is an entity. A piece of ordinary matter is made up of leptons and baryons in fixed numbers.

The mass of an atom at rest is a form of energy, but matter may be something extra.

This leads to one more instance of broad ideas derived from physics, my last. It is one of the strangest facts of Nature that she is so fond of whole numbers. The mere existence of large classes of individuals identical in each class such as electrons, protons, atoms of fluorine, molecules of ethyl alcohol and many more is surprising enough. One might expect a continuous gradation of sizes, weights and charges. But it is not so. On the smallest scale only certain types of particles exist. The next most complicated things, atoms, are built of integral numbers of electrons, protons and neutrons. The numbers are fundamental and characteristic.

An atom of carbon is a pattern based on the number six, there are six electrons, six protons and six, seven, or eight neutrons according to which of the three kinds of carbon atom it happens to be.

This is such a commonplace of science that one is apt to take it for granted. But surely this, and similar facts about other atoms, are most startling if one thinks. Further, the details of the pattern made by the electrons depend on another set of small integers, the quantum numbers. We cannot say yet whether this is also true of the arrangement of protons and neutrons in the nucleus, but there are indications that it may be. We are getting back to one of the earliest scientific ideas. Pythagoras taught that whole numbers are supreme.

It is worth notice that the masses of the atoms, even reckoned at rest and in the lowest energy state, show only rough regularities. The energy of formation and the mass representing it is variable, and though energy is sometimes divided into units there is here a continuous variation, for the unit is $h\nu$ and ν can have any value. Yet even here we have units in a sense and all photons of the same frequency have the same energy.

Molecules again are composed each of an integral number of atoms. It is true that at the moment of a reaction it may be difficult to say which atoms belong to a molecule or even if the concept of molecule is valid, but few would deny its importance in general. It is a concept which, though it owes something to the desire of the human mind to simplify, yet arises almost inevitably from nature. If the Martians are competent chemists they will, sometimes at least, think in terms of molecules.

Rather the same can be said of the concept of a cell. Living matter is generally speaking cellular, and the cells that compose a given kind of tissue are mostly pretty much alike in size and shape. Even the organisms that they form tend, at least, to fall into species each containing very many similar individuals. In many cases also the component parts of an individual are multiplied, e.g. scales or leaves.

While the 'atomicity' of electrons is perfect in the sense that each is in principle indistinguishable from any other, and the possibility of exchange without resulting difference is built into the theory, the atomicity of members of a species, of cells, and even of molecules is less rigid, but concepts embodying it are highly useful and represent something real in the world.

At present one can only speculate as to how far the complete atomicity of the elementary particle causes the partial atomicity one sees in biology, or for that matter in astronomy. If it is *not* a sufficient explanation then we must look even deeper than the elementary particles for this principle of atomicity, which would make it very fundamental indeed.

I have tried in this Address, in a very discursive fashion, to remind you of a few of the ways in which science has provided and is providing new ideas tied closely to experience, though often experience of a special character. These ideas constitute an achievement of which man may well be proud. It is surely something for beings, so utterly insignificant compared with the smallest of the stars that are scattered with reckless abandonment in the heavens, to be able to understand some at least of the principles which control their existence and enable us to perceive them.

To see these principles as applying equally on earth, as manifest in the most varied phenomena: in the motions of the tides, in the blue of the sky, in the lightning flash, and in the falling apple: to prove our understanding by creating, on however small a scale, compositions of our own which use these principles in new ways of our own devising: to be beginning to see some light on the nature of living matter and how living forms can transmit themselves to descendants, all these are worth while and worthy to rank with the achievement of sculpture, of music or of literature.

Science is not merely the control but also the understanding of nature. Its two aspects must be held in equal honour.

CHEMISTRY IS NOT ENOUGH, OR THE INCREASING SCOPE OF SCIENTISTS' WORK IN INDUSTRY*

By Dr. JAMES TAYLOR, M.B.E. *Director of Imperial Chemical Industries Ltd.*

Chemistry is not Enough

It's wonderful to be a scientist! It's even more wonderful to be a chemist. Think of the exciting moments in the laboratory when a new discovery is emerging and when the last few pieces of the jig-saw puzzle fall into place. Even the long patient discipline of experiment and routine can be comfortable and happy. How attractive it is to work and dream in such an ordered cosmos where logical scientific pattern prevails; where we can set our own fascinating problems and pursue truth 'as and where the inspiration comes'.

For better or for worse, however, we are living in a society which is based physically on Western science and technology. This has inescapable consequences for the scientist. Initially he was an accidental force in the unfolding of human destiny. This he can no longer be. As Niels Bohr has said, scientists must become conscious of the fact that they are not merely observers, but also actors on the stage of life. To the everyday world crowding in upon us, science is only a means to an end, and not an end in itself. Science is not enough. Chemistry is not enough. Scientists must live and even chemists must eat.

Modern societies are preoccupied with efforts to improve their material conditions and they are doing this under such intense competition that the less efficient will either fall behind or go to the wall. The means which are used depend on the application of scientific knowledge. Only scientists have the necessary training for establishing and using the technologies required to exploit recent major discoveries and those likely to arise in the future. It is clear that they will be brought more and more into technology, and thus into industry, the vehicle by which mankind exploits science for his own ends. It is certain that Huxley's view 'that the chief aim of the scientist should be to increase material knowledge rather than human convenience' cannot prevail in modern society which will be forced to deploy its scientists in a manner conducive to fulfilling its objectives. Moreover, since the scientist is a part of society he will be impelled to respond to the needs of the times and assume a wider rôle.

Adaptation to this technological way of life is not an easy process. The tempo at which events are moving is very quick. Changes, which previously took hundreds of years, now take but a year or two and there is no longer the opportunity for gradual adaptation to them. We are confronted too with the spiritual sickness which so often seems to accompany a high standard of living.

An understanding of science is not intuitive and can only be acquired by deliberate education. The scientist passionately believes that the education and disciplines of science are of great merit and potential, *but* he must come down from his ivory tower and play a full part in developing the 'good life'; not merely as a narrow specialist supplying the data of invention, but in the whole complex of problems which confront us. He must rid himself of the idea that a scientist who goes into business, or a chemist who becomes a salesman, has let down the side and prostituted his heritage, and develop to the full the contribution which his methods of thought and integrity of approach can make in all the multifarious activities of industry.

Finally, in the wider world of politics and affairs where as yet he has neither been accepted, nor

* Address delivered to Section B (Chemistry) on Thursday, September 1, 1960, at the Cardiff Meeting of the British Association.

indeed applied himself, he must deliberately reach out and become an active force. This is his destiny; to this end he was born.

The World we live in and the Scientist

In our modern world man is exploiting the increased command of his physical environment and is pursuing science for utilitarian ends to improve his standard of living.

The phrase 'standard of living', which we use so often, needs examination. In one sense it is simple and understandable when it refers to having a reasonable amount of food, adequate leisure, good health and living accommodation, but it involves, and always has done, much more than this. It includes many wants which are an extension of the primitive instinct for decoration, ritual and culture and, in the widest sense, those things which we recognise as pertaining to 'the good life'.

Our life is based on energy. Advances in living standards have hitherto been brought about in two ways—by man learning to harness new sources of energy to supplement the extremely restricted supply possessed by himself, and learning to use more effectively and economically the supplies, including his own, which are available to him.

It is generally accepted that the standard of living is directly proportional to the energy which is usefully available per member of the population. In prehistoric times when man was a simple food-gathering animal it took all his energy to keep alive, but as he devised means of using energy sources outside himself he achieved a progressively higher standard of living.

It is no longer possible, as it was formerly, to discover new sources of energy by accident or intuitive invention. There appears to be little doubt that energy from nuclear reactions will in due course provide unlimited power, but as Heisenberg says 'atomic technology is exclusively concerned with the exploitation of natural forces to which there is no entry at all from the world of natural experience'. Future advances will be derived from research carried out by pure scientists.

The mere utilisation of energy does not itself provide the conditions for unlimited leisure because people become machine-minders and clerks, and physical drudgery is replaced by mental drudgery.

The possibility of great reduction of mental drudgery is in sight. This has been brought about by a new and, in a way, rather frightening revolution which is now upon us, based on the application of all those techniques collectively described as automation. These techniques, operating through the much publicised electronic brains, provide means for designing machines to supervise machines and to do a great number of things hitherto considered as capable of being carried out by human beings alone. In the future, fewer machine operators and clerks will be required and the possibility of a very high standard of living, combined with a great deal—if not unlimited—leisure, is foreseeable.

Again, these are techniques to which only the scientist has access.

The chemist has played a great part in providing and improving sources of energy, but his peculiar contribution is to 'the good life'. Clearly there is the task of providing the basic substances, such as fertilisers for increasing food production, medicines and antibiotics for improving health and vigour, and new materials of construction. There is also the task of satisfying man's—and more particularly, woman's—need for decoration and beauty. The inventions of rayon and modern dyestuffs have, in a few decades, brought about a revolution in female dress and put within the means of everyone fashions which were previously available exclusively to the rich. One can indeed produce a somewhat frightening list of contributions. We travel along M1 at 100 miles per hour, or more, in fibre-glass-polyester motor cars propelled by high-octane fuels, listening to radio sets transistorised with high-purity germanium and silicon, reclining on P.V.C. upholstered seats, firmly, but gently, supported on polyurethane foams and, if we are lucky, accompanied by our ladies, clad in acrylonitrile and polyethylene-terephthalate and decorated by some of the more sophisticated products of the dyestuffs organic chemists. We eat our sandwiches from polyethylene packs with polymethylmethacrylate dentures.

We may laugh at this extravagance and claim that we are really no better off for all these; that they are no basis for a cultured life. Personally, I beg leave to doubt. I cannot see why a Persian carpet is considered to be a work of art and evidence of the good taste of the owner, whereas a nylon

dress, or a P.V.C. fabric, is merely the expression of a 'false need'.

I have touched upon the scientific and technological contributions which can be expected from the scientist and which extend far beyond his activities as a pure searcher after knowledge. But we do not live in a logical and ordered world and are still dominated by forces which are illogical and derived from man's remote past. We recognise, although we may deplore the fact, that we live in an international jungle and our first necessity is to survive in it.

Britain's Place in the International Jungle

Man has always lived in a jungle ever since he came down from the trees. The essential characteristic of a jungle is competition. The painted savage wants more food in his belly than his brothers; Mrs. Jones wants a bigger ice-box than Mrs. Thomas; and Mr. Krushchev wants a bigger I.C.B.M. (Inter-continental ballistic missile) than Mr. Eisenhower. That's how it is!

Competition is the flowering of the law of survival. Individuals, families, organised groups, countries and nations all compete for a living from restricted resources. Nowadays, competition assumes unusual and sophisticated forms, but the struggle for existence goes on. Industry provides the means of carrying out this struggle. Its tools and techniques are increasingly scientific.

We in Britain have built up our standard of living by international trade and commerce. The people of Britain have never been so well off and the slogan 'you've never had it so good' is a factual statement, even if it has been used for political purposes. To quote Professor C. F. Carter, 'what is special in our situation is that we have achieved a large, densely settled and prosperous population on an area of limited resources. No other great industrial power has grown so strong on resources so limited.' Our success has been derived from industrial development employing our native skill, energy and enterprise, but, quoting Professor Carter again 'since we live in a world in which others are energetic and skilful, our chance of keeping our place in international competition depends on what we can do to stimulate energy, to extend knowledge and to develop skill'.

Put into plain words, our position is precarious because we have no monopoly of brains or creative ideas. We shall certainly need all the help that scientists can give us.

Industry

Our success has stemmed from industry which is responsible for the whole material well-being of the country and consequently merits our consideration. The chemical industry is particularly important because of the great contribution it is making to our prosperity. Since the war it has become a major world supplier of chemicals. Exports are currently £280 million per annum which gives it third place in our list of exports.

The notable feature of the present century is the growth of the great scientifically based industries. These are not casual developments but have arisen out of the needs of the time; the result of the interplay of the forces of business and science.

A great deal of nonsense has been written and said about industry and much of it is half a century or more out-of-date. I visited Russia three years ago on business and found a very great interest in my own Company. I was asked if the Company were owned by the Chairman, and it was quite evident that they had imagined him as a super slave-driver cracking a whip. They were genuinely sorry for us. We explained our organisation and objectives and they were amazed to learn that the Company was owned directly and indirectly by thousands of 'little men' and not by any one man. A day or two later I saw sticking out of the pocket of one of our Russian hosts a newspaper with a cartoon of a Western industrialist in a top hat smoking a large cigar. Seeing me looking at this he was quick to point out that they didn't think we were like that, and indeed they understood that we were managers of industry very much the same as they were. This is true. Most large industries are run by men who, like all the other employees, are servants of the Company, and they have no incentive to unfair exploitation of the employees, the customers or the shareholders. Industry has developed a sense of public responsibility and accountability which in my view is to no small extent due to the influx of considerable numbers of scientific and technical people. Contempt of industry is out-of-date and outworn.

Industry exists to supply the goods and services the public requires. It presents both a challenge and an opportunity to all the skills of the nation

Further, it must operate at a profit or it cannot maintain, re-equip and modernise its plants and service its capital requirements. There is much misrepresentation about the profit motive. In fact, the maximum possible profit and participate directly in a cash bonus and improved amenities.

The object of efficient industry is to produce the goods and services required with the least expendi-

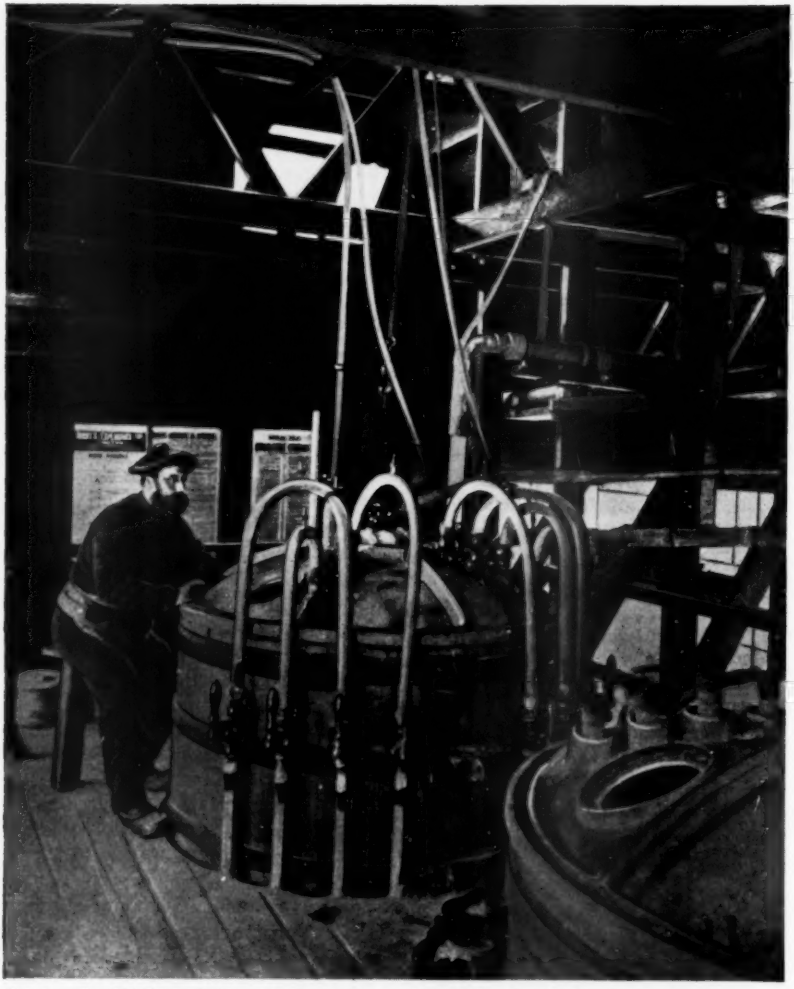

Fig. 1.—*The Old Plant.* Old sketch of a nitroglycerine batch nitrator showing worker seated on a one-legged stool watching the thermometer as the glycerine is added.

it is a perfectly understandable objective whether in private or nationalised industries and, indeed, in my experience is well understood and appreciated, even in Russia, where all factories are out to make ture of human effort and this means as cheaply as possible, since human effort is measured in terms of money. The consideration of economy of effort must be applied not only to direct labour, but to

plant, buildings and equipment. This is evident when we recall that plant and equipment, however automatic it may finally be in operation, has been fashioned by human labour and its capital value represents an accumulation of human effort. In this object there is a worthy outlet for the energies and skills of the scientist.

The Chemical Industry

The chemical industry was built up in Britain during the industrial revolution. For a long time it was dominant, but was overtaken by that of Germany where, owing to the provision of opportunities for scientific training, the manufacturers could tap 'a large reservoir of extremely capable chemists'.

In Britain it was much later that scientific training, fortified by the growth of the provincial universities, and management perception, produced an analogous climate.

The organic chemist was adequate for the needs of most of the industry as long as it was manufacturing simple chemical substances, but with the advent of the plastics era and of polymers which were not single chemical entities, inorganic and organic chemistry no longer sufficed, and scientific knowledge of the control of the properties of substances had to be sought by the application of physical chemistry.

In the modern chemical industry it is essential to employ not only specialists, but also scientists with a widely-based training in fundamental scientific principles and methods, who can appreciate what is technologically possible and desirable in plant design. Products, such as those from the Fischer Tropsch and the 'OXO' processes, and, indeed, all petrochemical products, require the application of techniques and instrumentation far removed from simple chemistry. This is also true of the other recent developments, for example, high density polyethylene and isotactic polyolefines, and still more so of the 'newer' metals like titanium and beryllium and of ultra pure metals or elements produced by zone refining.

To quote Heisenberg again, 'Chemical technology might still have been considered as the continuation of old branches of handicrafts; we have but to think of dyeing, tanning and pharmacology, but the scope of chemical technology, as it has evolved since the turn of the century, no longer permits any comparisons with earlier conditions'.

I entered the chemical industry thirty-two years ago. By that time a number of large scientifically competent companies had emerged by the process of integration brought about by economic and technological necessity, particularly in Germany and America, but also in Britain. Modern synthesis of chemical products with all its complicated techniques had started.

In those days chemical manufacturing companies were operated by technical staff who, however, were in the minority on the Boards, which were comprised predominantly of 'men of affairs'. Today the position is reversed.

Thus, while formerly chemical industry was directed and controlled by men of non-scientific education, nowadays their place has largely been taken by men with scientific educations. Equally important, such men have achieved, in considerable numbers, positions of general and administrative responsibility.

The Academic wets his Feet

I have dealt with the technological requirements of our age and indicated the increased burdens which have been put upon the scientist. It is important for us to examine why and how the scientist has come to accept this rôle of serving 'human convenience' as well as his traditional rôle of increasing material knowledge. As far as this country is concerned, when we are talking of the scientist, we are really talking of the Academic and we must consider, therefore, what forces brought him into technology and industry. What made the Academic wet his feet?

The British Public has a profound belief in the capacity of the University Professor and the Academics to solve its problems when in trouble—as in wartime—and in true democratic fashion this belief is held by Politicians. To most people it must therefore be a matter of surprise that the employment of academically trained scientists in British Industry is a fairly recent innovation.

The entry of science into industry has been a long and slow process. Intensity and rapidity of application are characteristic of recent times.

The most disruptive influence on the academic life in the present century has been war. Modern war makes a large demand on the scientific and

specialised talent of universities. Academic scientists were employed in great numbers in both world wars and made many notable contributions. The Kaiser's war was a chemists' war, whereas Hitler's war was a physicists' war. Both forced university scientists into technology and industry, but the impact of academic chemistry occurred earlier than that of physics.

Another disruptive influence was economic, particularly during the slump of the 1930s. However much university students believed in the pursuit of knowledge as an end in itself, like everyone else they had to eat.

The pursuit of 'truth' is an occupation of privilege which can only be carried out by a minority relieved of the responsibilities of finding their daily bread, which perforce must be the lot of the majority. Academic privilege is derived from, and supported by, our industrial activities. There were no academic pursuits when man was purely a food-gathering animal whose living depended solely on his own efforts.

We are now turning out considerable numbers of science students and the bulk of them will have to earn their living in fields remote from the scholastic and academic; in factories, industrial or research association laboratories, or in Government employment. A considerable number of students who begin as scientists will finish as technologists or technicians. In my view they will be none the worse for this.

Education in Science and Technology

The realisation that we are living in a technologically based society has led Governments in this country and elsewhere to devote considerable attention to the provision of educational facilities to produce increased numbers of suitably trained personnel.

The universities for their part having only recently, and indeed only incompletely, attuned themselves to science, are now confronted with the task of adapting themselves to technology. Industry is in the process of adapting itself to the university-trained scientist and the orderly thinking of academic disciplines. These adjustments and adaptations are not carried out in the abstract, and it is the individual who is the vehicle of the reacting forces. Consequently, the most important task is that of education. The opportunities for young people in the future are better than they have ever been before and it is to be hoped that they will receive an education which will equip them to cope with, and enjoy an industrial society. Most young people who are entering industry today have not been so educated. I shall deal later on with the problem of adaptation in an industrial community, but before that I want to make a few observations about the scientist.

Why do people become scientists? It would be pleasant to think that scientists, like musicians, were born with a natural gift, but I'm afraid that there is little to support this. With few exceptions most people drift into science. It is mainly a case of early influence at school and whether the physics or chemistry master makes the subject live and exciting.

Early specialisation is generally considered to be a bad thing. Nevertheless, as far as the boys or girls at school are concerned, they must specialise, since otherwise they will not obtain the grants or scholarships necessary to continue their education. Students are often criticised for the shortcomings of this specialist education, as if they had a choice. The facts are that for over 99 per cent training is for a job or career rather than a liberal education.

There are people who claim that science graduates are both narrow and uncultured, but in my experience they are no more so than modern arts graduates. The fact is that 'culture' comes from a privileged background, usually the home, and many graduates being of humble origins have had no opportunities for absorbing a wider knowledge. Whilst the student has really no option but to work within the educational system which is available to him, it is good if he can be aware of its defects and take such steps as he can to correct them.

Students in science are trained by school and university teachers, who for the most part have no personal experience in industry—or for that matter outside their own specialised sphere. The approach consequently tends to be too scholastic in its fundamentals and not infrequently is flavoured with the conviction that anything which is of practical application is either suspect or unworthy. It is not surprising that the graduate, relatively mature by the time he obtains industrial employment, finds the process of adaptation both difficult and perplexing. Indeed, it is unlikely that he will

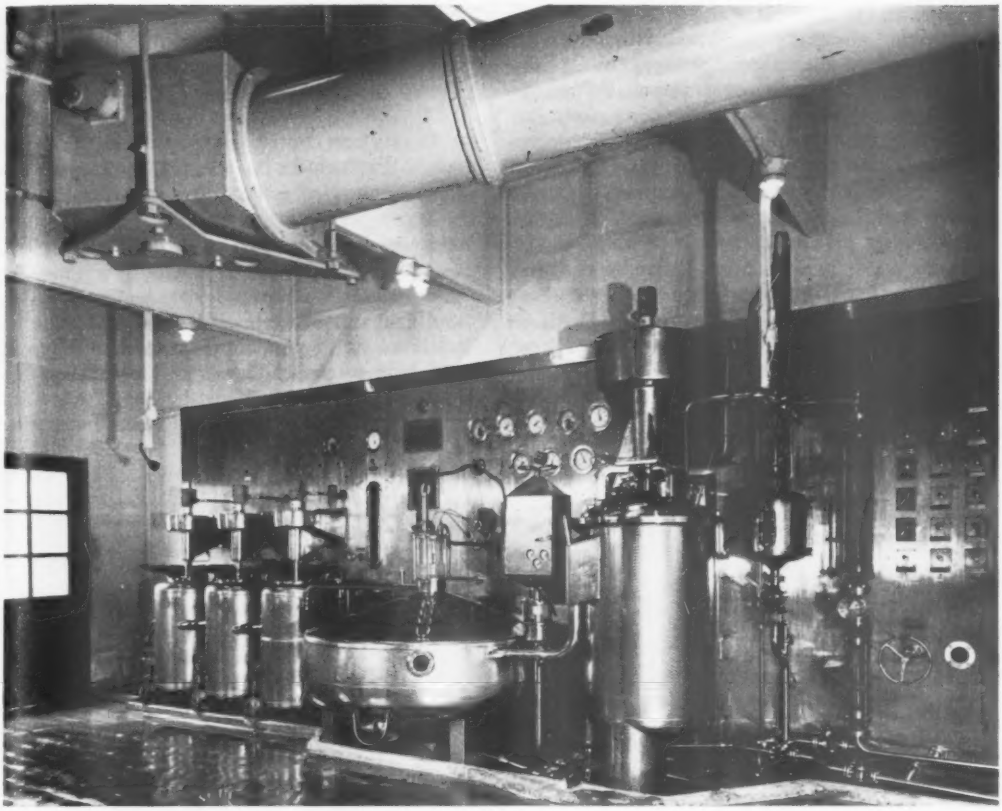

Fig. 2.—*The New Plant.* General view of a modern Nitroglycerine Plant.

appreciate that apart from any specialist contribution which he can make, there is a growing field of use for the disciplined type of thought in which he has been trained.

Professor Cottrell in his recent Inaugural Lecture in the University of Edinburgh gave some provocative views derived from a long period in the chemical industry, and concluded—'It is my assertion that we, as academics, have a duty to society to be academic. We are supported on a scale which is only justifiable if the academic attitude has a valuable contribution to make to society. I believe that it has, and that the academic attitude with its insistence on theory, and its critical and intellectual approach, is distinctive and important.'

Adaptation in an Industrial Community

The change over from a type of industry with simple manually operated plant to one with mechanised and automatically controlled plant is a process involving many problems.

For the workpeople the change is no simple one. It is difficult enough for people of advanced education to adapt themselves to new ideas and ways, but it must be much more difficult for those who have left school at the age of fourteen to make the transition from a well-established routine to a fundamentally different method of working. Often too, such changes have social consequences which may disrupt the established habits of the local community. It is surprising, therefore, that British workers have in fact proved to be so adaptable. I

might give one example which I think well illustrates this. Recently I visited an Explosives Factory where the modern nitroglycerine plants are remotely controlled, and viewed by closed-circuit television, or optical system, from an instrument room (see Figs. 2 and 3). The process-man operating one plant told me he was retiring after over forty years. This operator's service almost went back to the days when the plant was the simplest possible and the worker sat at the nitrator on a one-legged stool thereby ensuring that he would fall off if he went to sleep (see Fig. 1). Here was a man who had adapted himself to the most revolutionary changes and who had done it so thoroughly that he much enjoyed the new methods and was sorry to retire. We could all learn a lot from his example.

It is clear then that many workers, perhaps most, have reacted to the changing conditions and modified their outlook accordingly. This has come about with little if any direct help from school education and what assistance they have had has been derived fortuitously from everyday contact with modern products, such as motor vehicles, radio and television. There is little doubt, however, that this adaptation could be deliberately helped by suitable primary education and the employment, as process workers, of young people of higher educational standard than has hitherto been customary or possible. If this is true of process workers, it is even more true of tradesmen who will have to maintain and service complicated plant. It is one thing to instal modern plant but quite a different thing to run it. That, of course, is one of the prime difficulties of relatively under-developed countries, such as India.

My own experience in the explosives industry serves to illustrate this. Twenty years ago there was not available a corps of skilled electricians, mechanics and instrument artificers capable of maintaining and servicing the mechanised and remote controlled plants which are now in everyday routine use. Since then an adequate force of technicians has been built up. These men have developed a pride in such work and an appreciation and understanding of its objectives. The training of specialist personnel takes time, however, and is one problem of the nation's adaptation to the new era. A much clearer understanding of the problems of adaptation in an industrial community is needed and a more positive approach to the education problems involved is overdue. In this field I believe the scientist could make a contribution.

The Chemist

We may now consider the part which the chemist, and that includes the scientist, can be expected to play in the chemical industry in this country.

To prosper in this competitive world we must have an adequate supply of new discoveries and ideas to provide for future progress.

Hitherto basic discoveries have but rarely arisen from deliberate work towards industrial purposes and if we are to promote them in the future, we shall have to ensure that there are sufficient individuals and institutes with 'the freedom to gather fresh knowledge as and where inspiration comes'. Universities are the places where traditionally this freedom is jealously guarded and where the search for 'truth' or knowledge for its own sake is the primary object, and it is to them that we must look in a large measure for the fundamental discoveries of the future.

The Universities have already made many contributions of great practical importance, as, for example, Clark-Maxwell's theoretical studies in electricity, J. J. Thomson's researches in electron physics and Rutherford's work in nuclear physics; and in the chemical field Perkin's work on dyes and Kipping's on silicones. Nevertheless, many major advances came from elsewhere—Faraday's experiments in electricity, Pasteur's researches on brewing and microbiology and Carruther's discovery of nylon.

It is becoming increasingly difficult for individuals to make and develop new discoveries and inventions. This arises particularly from the fact that the means of carrying out effective research are becoming more and more complicated and costly, and require large teams. Even where the basal idea may be individual, as for example, Whittle's idea of combining the turbine with the reaction jet, the development is expensive and needs great resources not available to individuals. It is likely that in the future we shall have to rely to an increasing extent on the professional who has behind him the resources of a university, a research institution or industry, to provide the continuity of discovery and progress of scientific ideas which is so essential to the nation's prosperity. The

contributions already made by national and industrial laboratories in this and other countries, are impressive: We need only mention the discoveries by Coolidge and Langmuir in the G.E.C. Laboratories in U.S.A.; the work on polyethylene by I.C.I., on resin exchange in C.R.L. and on terylene by C.P.A.

We have a responsibility to provide suitable conditions in which scientific imagination and invention can prosper. Unfortunately the conditions for promoting originality in scientific discovery are not known. Certainly a considerable number of promising research chemists must be given freedom to pursue their studies without ties or pre-selected targets, and their work must be judged by its scientific quality and not by its immediate practical value. This necessitates responsibility, accountability and a dedication of purpose on the part of the chosen scientists.

It must be emphasised that outstanding discoveries occur but rarely and that they cannot be forced. Any nation, therefore, which commits its future to science must have the quality of patience, and not be perpetually digging up the plants to see how they are growing.

If we are to be quick at exploiting chemistry to our advantage it is essential that we should appreciate the possibilities of scientific discoveries, not only if they are made by ourselves, but wherever they may occur. This requires an

Fig. 3.—The Control Room of the New Plant.

unprejudiced open-minded attitude at variance with the present tendency to extreme nationalism. We must cast our nets widely and where necessary be prepared to 'buy in'. It must also be borne in mind that many entirely new industries arise from inventions, as for example, the Zip Fastener industry, or from the appreciation of the possibilities of existing scientific knowledge and its development by applied research, as in the case of Nobel's exploitation of nitroglycerine and fulminate of mercury for the explosives industry. The exploitation of existing knowledge and filling in of gaps are important functions of the chemist.

Discoveries are only knowledge. *Per se* new ideas and conceptions are not immediately useful. They can only be exploited by technology, and progress is beset by considerable difficulties. The laboratory discovery is only one event, albeit an important one, in a long train which culminates in successful large-scale manufacture. As a colleague of mine once wrote in a plagarism of Longfellow:

> First the beaker, then the pail,
> Then the semi-technical scale,
> Next the plant and then disaster,
> Swiftly following faster, faster.

The follow up after the initial discovery involves applied research, development and chemical engineering, in all of which the chemist plays an important part. A project is a complex, but orderly process, in which scientific, technical, engineering and financial staffs all play essential parts in a combined operation. Adequate scientific data are necessary for its inception and design, efficient technical resources for its construction and sufficient funds for financing it. All these aspects are equally important and, any one, if neglected, can imperil the whole.

I do not propose to dwell further on the techniques and procedures for capital projects now used in the chemical industry because others will be dealing with these matters. I would, however, like to mention some other activities in which scientists are making an increasing contribution. Work study, operational research, analysis of requirements and the study of overheads are fields in which science graduates are being increasingly used. There are also chemists and men of other scientific training in financial, commercial, sales and personnel positions throughout the chemical industry and thus for the first time the scientist is in the position to fashion the ends as well as the means. This has had the result that the scientific approach is being increasingly applied. Once the climate is favourable to such an approach the benefits extend because the methods, apart from their specialist and technological applications, are simple and can be applied by men of non-scientific education and be understood and accepted by workers. What varies is the 'skill' and not the techniques.

Personally, I like to think of the scientists' contribution to industry as a philosophy of 'approach'. It is the application of a particular mental attitude which can be shared alike by the scientist, the engineer, the financier, the salesman and, in the final analysis, the workman. It is the logical conclusion to the affinity of science and business, quoted by Sir Alexander Fleck in his Presidential Address to the British Association, 'The power that was science and the power that was money were, in final analysis, the same kind of power: The power of abstraction, measurement, quantification'.

THE ORIGIN OF ANGIOSPERMS*

By Professor T. M. HARRIS, F.R.S. *Dept. of Botany, The University, Reading*

Presidents are given to reviewing their subject broadly, and their audiences must half expect to be made to look backwards on past achievement, and I think this is no bad thing—on rare occasions. But I ask you to look back, not on a proud record of the success of famous men, but on an unbroken record of failure, and I think it will be kinder if I leave out the names of the famous men concerned. It is, indeed, a record of vigorous and even gallant failure and of what seemed heartening discoveries, that were perhaps only proved irrevelant many years later and after the discoverer's death.

Botanists have approached the origin of the Angiosperm from many sides and along lines of thought so different that they hardly aroused disagreement, but when two are close, disagreement is the rule. I shall limit myself to my own special interest, that is, fossil plants, and, indeed, with real fossils which may be or may not be ancestors of our flowering plants, or, if not that, cousins of ancestors.

Now, you may remember that this problem worried Darwin and, I suppose, a lecturer in 1860 could have summarised knowledge in almost the words I now use. Fossil Angiosperms or flowering plants are abundant and familiar. We can trace them back with ease in suitable rocks as far as the Cretaceous period and then a little above the Middle of the Cretaceous there are none or very few, virtually none. The more familiar fossils of periods older than that are most unlikely to be related to the ancestors of flowering plants: that is not to say they have not been considered as ancestors, they have, and vigorously, but they are too different and are unacceptable. The reward of their study has been to bring to light many strangely organised plants and I suppose the quest for the early fossil flowering plant has been the driving force that has led to a good deal of our knowledge of the ancient plant world. Of course I believe the Angiosperms had an unbroken series of ancestors before the Cretaceous. And I have faith in the possibility of scientific progress; I believe that they are represented by fossils which are within the wit of man to understand. Let us for a moment turn from these early ancestors and consider what will happen to botany when they are effectively brought to light. I think the impact will be felt widely.

We have, of course, our ideas of primitive Angiosperms based on the study of the ones of today. We have, indeed, so many ideas that one group of them is almost sure to be nearly right, but the trouble is that among far too many rival ideas we have no means of selecting the ones that are right, and so we distrust the lot, and with distrust comes aversion. Then, as you know, plant classification is based, not on mere convenience, else we should use something like Linnaeus's scheme, but on an idea of perfection; we aim to put together those plants which have closer blood relationship and to separate plants of another family (I use the word in the human sense) whether they look similar or not. Since we do not *know* what plants are really related (apart from the very closest) we can only argue and use intuition and these have led to the diverse results that plague the arrangement of herbaria and of Floras. I believe a moderate number of good and convincing Jurassic Angiosperms would kill all theories but one. Again the comparative morphologists would find themselves agreeing about many of their morphological series, and could go on to something else. Variety of views and conflict are good, but when there can be no decision, the thoughtful man is discouraged and

* Presidential Address delivered to Section K (Botany) on September 1, 1960, at the Cardiff Meeting of the British Association.

goes elsewhere and so I think it is to a large extent with the comparative morphology of the flowering plants, but you may doubt if these fossils would have so great effect on thought. I think I can convince you, with two smaller groups, that fossils have caused men to think along similar lines and in the same directions as one another.

The first group is the ferns and the work was done before my time; so that already when I was a student there was substantial agreement about many things, for instance that the massive sporangium of Botrychium is more primitive than the small one of Polypodium; that the protostele is more primitive than the dictyostele and so on; you will find it all in Bower's book. Now I cheerfully admit that all this could well be put forward entirely on the basis of the comparative morphology of recent ferns, and, indeed, it very largely was so; but so were other views which are now forgotten. The fact that the views of Bower and those with him prevailed is very largely because they could, and did, freely use the evidence of fossil ferns to give a direction to evolution in morphological series. In fact I suppose there is no-one who would disagree that such ferns as the royal fern, *Osmunda*, are among the more primitive tenth of ferns now living today; and this is due to the labours of fossil botanists. The fact that the classification of the more advanced nine-tenths of modern ferns is in considerable turmoil is related to the fact that the fossil botanist of the late Mesozoic and the Tertiary have not given the fern men the right information.

Then there are the conifers. When I was a student there was unresolved, and as it seems to me hopeless, diversity in this rather small group. It all centred on the seed-bearing scale, and I suppose I learned for examination purposes six or more varying theories, all doubtless the fruit of thought of able men. Then at about the time of the last war it was all resolved by Florin's comprehensive work on the Carboniferous Conifers which are the oldest known. Then at last one view prevailed and the others quietly vanished. It was no compromise—Florin's findings supported an extreme form of one view of the comparative morphologists. I do not say Florin must be right, I merely say that he has given us a body of evidence that a man can understand and respect. He has given us peace to get on with our work. How grand if I could say this of the flowering plants!

I return to my statement that the Angiosperms arose to dominance, suddenly, rather after the Middle Cretaceous. I should rather have said relatively suddenly. If you make a diagram of the rocks of earth's crust as layers one on top of another, like layers of a cake and make the layers thick according to the time they lasted, and if you think merely of the plant world, then there are just two main layers in the last 180 million years since the floras of the Palaeozoic vanished. The period while the Angiosperms were increasing from just a few to dominance may have lasted a million years or so, but it looks like a dividing line on an ordinary diagram.

We just do not know why the change happened. We are fairly sure no other stirring event occurred in the world at that time. Here the chalk sea was depositing chalk—steadily—before, during and after the change and the animals of the chalk sea continued their quiet evolution. On land the dinosaurs remained and the mammals had to wait in the background. There were no world-wide terrible volcanic eruptions or submergence of continents, or ice, or desert ages. Indeed, when terrible things happened, as in the Tertiary when the world's mountains were built or in the Quaternary with its ice ages; the plant world seemed to take the changes and adapt itself easily.

No—it's no good looking at other things for an explanation. The botanist must solve his own problems and I think that the fossil botanist must look at the early Angiosperm to understand its success, that is, of course, when he has one to look at.

Now all this is textbook stuff, let us look afresh at some fossils.

First, how do we recognise a fossil Angiosperm? The answer is simple, we match our fossil, that is the unknown, with something we know well. With fossils we nearly always have to deal with isolated organs—leaf, seed, wood, pollen grains and so we are just matching single organs. Take leaves. For all their variety, most Angiosperm leaves do conform to a few basic patterns which a very ordinary mind can grasp. Thus if you meet the flora of a strange country, and a fossil flora is rather like that, you will hardly ever be in doubt about whether a leaf belongs to an Angiosperm.

I say this from knowledge. I have walked up a mountain in West Africa and have seen perhaps a thousand species I did not know at all, and for many not even the family; but there was not one which left me wondering whether it was an Angiosperm. There are very few plants so odd that a botanist would be fooled if he did not know that genus. I would say Duckweed is one, and a Dodder without its flowers, but not many more, I think.

This is equally true of fruits, seeds, petrified stems and of pollen.

Now when I match a fossil organ with a known organ and then say they agree so well that I conclude that the whole plants are also similar, I am expressing not a certainty but a conviction that it is probable. It is like betting.

I am making a bet because I think the probability is that I will prove right. I cannot calculate the odds, I have a hunch based on doubtfully cogent evidence. I cannot be sure, and if I express myself with vehemence you will suspect that I am bolstering myself against doubt.

If I have one of those better known fossils with two known parts, say the leaf form and the stem anatomy, my case is considerably stronger, but it is still not certainty. Remember that troublesome Gymnosperm *Gnetum* with leaves like a typical dicotyledon, in fact like a laurel, and with a stem like a dicotyledon too. It is only when our fossil becomes a whole plant and as good as a recent one that we have a right to call ourselves certain. If we must wait for this we shall have to wait a long time, and we shall not wait, for fossil botanists are not timid in these matters.

Now before the dividing line there *are* fossils with claims to be taken as early Angiosperms. And they are all kinds of fossil organs—leaves, wood, seeds, pollen, but as you will see they are an unsatisfactory lot.

I prefer to regard them not as pre-Cretaceous Angiosperms but as candidates for election to that very exclusive club, enthusiastically proposed, indeed, but whose credentials are not yet sufficiently examined for admission; so that so far the club has no members. What *has* happened is that quite a large number of candidates have been proposed in times past, the credentials have been examined and the candidates have been rejected. We will consider that this admits them to another club, rather distinguished, but not exclusive, the club of exploded fossil Angiosperms.

There may have been a dozen candidates as early Angiosperms in 1860, and probably all are exploded and forgotten, and many that have been exploded since, and these are among the respectable fossils of other classes described in textbooks. There may be a dozen candidates today whose fate is undecided; I would be surprised if all gain admittance.

Let me first give you some candidates that still stand. The first is a leaf from the Rhaetic of East Greenland; rather like a large willow leaf, except that most specimens fork; it is called *Furcula*. I select it rather than one of a few other leaves not because I described it, but because I need not be careful of its author's feelings. It not only looks like a dicotyledon; it has veins like a dicotyledon and epidermal cells like those of dicotyledon. I can say with assurance that had it not been so old, but instead had it come from some stage of the Tertiary nobody would have doubted that it was a dicotyledon; the only problem would be—in what genus shall we put it? I am told that it would fit well as one of the Sterculiaceae; one of the many families with which I am poorly acquainted. Since I described *Furcula* over twenty years ago, the Russians have also found it in their country, but unless they have learned more than I am aware of, the evidence that *Furcula* is an Angiosperm is exactly what it was. It looks like an Angiosperm; it does not look like any of the contemporary Gymnosperms known to us. That is what you get from forward comparison.

But you should also look back. There is an older leaf we call *Gigantopteris* which is found commonly both in China and in Western United States. We know little enough about the *Gigantopteris* plant, but current theory, based on a slender chain of evidence, supposes that it is a Pteridosperm.

Now a typical *Gigantopteris* leaf does not look like *Furcula*, but among some of the kinds recently described from East Asia are some that approach it in form and some that approach it in veins; I do not know about the cuticle. *Furcula* could be the last appearance of the *Gigantopteris* alliance. What do you think? Your guess is as good as mine.

Let me show you a hopeful candidate from a Triassic of Arizona. It looks very like a Monocotyledon; in fact Brown who described it

compares it with a young plant of a palm; a coconut seedling, for example, has leaves looking like this. Unfortunately no fine details are preserved, we have only what Brown's figures give and since his account is temperate, I for one, find him convincing. But is it a Monocotyledon? It looks reasonably like one; and it does not look like anything else, but there the facts end. Your opinion about whether it is a true Monocotyledon is as good as anyone else's. By 'whether it is a true Monocotyledon' I mean whether further knowledge, say of stem anatomy, will also look Monocotyledonous.

Let me take an exploded candidate and for ease in saving feelings I will take one of my own. It was fortunately never published, but I showed it jubilantly to a fair number of people.

It is a tiny crumb of wood preserved as charcoal, a millimetre cube. I extracted it from a mud-filled crack in a quarry in South Wales, and along with this fossil were thousands of fragments of the Rhaetic Conifer *Cheirolepis* also preserved as charcoal. A broken radial surface of this crumb of charcoal shows what appear to be vessels. The other broken surfaces were confusing and I did not bother with them at first.

Then I did what I should have done earlier, I forced myself to relate this radial surface to the other surfaces; the vessels should, of course, be seen in each. And it all broke down. My vessels were in fact rays distorted into round, open tubes; what I took to be rays were tracheids; I had my ideas wrongly orientated. It was, in fact, a distorted bit of *Cheirolepis* charcoal and I found I could produce the same distortion by heating fresh conifer twigs under sand till they were well charred. I think it is an ordinary distortion produced by ancient fire.

I now turn to pollen, the great recent development of fossil botany. It simply is fact that most Angiosperm pollen grains can be distinguished with more or less ease from all known Gymnosperm pollens and all known Pteridophyte spores. Angiosperm pollens are so diverse, and so are Gymnosperm pollens and so are Pteridophyte spores that this seems astonishing, yet so it is. Germ grooves and germ pores have only been seen in Angiosperm pollens and till they are demonstrated elsewhere it is reasonable to hold that they do not occur elsewhere.

Now pollen has this special advantage; it represents more or less the whole flora of a region, while fossil leaves are almost all the flora of a delta swamp or trees overhanging a river bank. If, as some have suggested, the early Angiosperms were abundant on the drier ground and the Cretaceous dividing line merely marked their invasion of the swamps, then we might expect to find their pollen in variety and in some profusion at early times.

What we do find is that the pollen behaves in a way nearly parallel to other Angiosperm parts. In the Jurassic and Cretaceous up to the dividing line it is at best rare; after the dividing line it becomes abundant. It is interesting that the change occurs in the middle region of the Cretaceous in many parts of the world; it is, for example, roughly contemporaneous in Europe, North America and New Zealand. Yet there are gleams of light as well. It seems clear that a moderate number of Angiosperm-like grains do occur in the Lower Cretaceous and their frequency increases as we approach the dividing line. There are, in fact, a few other Angiosperm-like fossils from the Middle Cretaceous but rather before the dividing line. I would mention some petrified Dicotyledon woods described by Doctor Marie Stopes from Bedfordshire forty-five years ago; and a few net-veined leaves from North America. If these scattered fossils proved to be all that is claimed for them, then they are the forerunners of the Angiosperm invasion. And they are varied and seem specialised rather than primitive.

On such evidence as this some people hold that the Angiosperms were already a great and varied class long before we first meet them in force. It may well be so, but I wish the evidence had been more critically examined; for it is a fact that very few of these early Angiosperm-like fossils of the Lower Cretaceous have been studied with the care they deserve. It behoves us to be careful with our evidence when we are going to draw conclusions.

There are already members of the club of exploded Angiosperms among pollen grains. The best and most interesting as well as the first is a grain described from the Rhaetic as *Tricolpites troedssoni*, a pollen grain with three longitudinal grooves, a frequent feature in Angiosperms but unknown in Gymnosperms. It has various details which give it fairly marked specific character, and

some years later it was found to range widely in the Mesozoic and well up into the Middle Cretaceous. It is evidently produced by a widespread and long-lived genus.

Then these grains were studied again by Dr. Couper working in Cambridge and independently and at the same time by Kuyl, Muller and Waterbolk in Holland. I will speak of Dr. Couper's work because I know it best, but the others reached the same conclusion. These grains, as usual, with early fossil pollen, are crushed perfectly flat, like a paper bag run over by a wheel, and you have to deduce the original shape from folds in a flat membrane, no easy task. Dr. Couper compared the proportion of grains showing the longitudinal grooves in one position and the number showing them in another and came to the conclusion that they could not have been radially symmetrical with three equal grooves; but had one broad major groove and two narrow minor ones. Now grains with one major groove are widespread both among Angiosperms and among Gymnosperms, and so they are not characteristic; what the minor grooves mean we just do not know; they give this grain specific character but do not show that it is Angiospermous.

Very recently, Dr. Hughes of Cambridge has announced that he found some little seeds with *Tricolpites troedssoni* in their micropyles. Their age is Lower Cretaceous, and we do not know at all what family of plants they belong to, but pollen in their micropyles makes them Gymnosperms by definition. I hope we shall soon be told more.

Dr. Couper has, indeed, exploded several other pollen species and I would merely observe that the thought and labour which went into exploding them probably exceeds that that went to their original proposal. So it is, inevitably. More recently several other Jurassic Angiosperm-like grains have been described and from Russia we have an announcement of one from the Carboniferous. I hope they survive fresh study.

I am no pollen man. I have noticed that people who are discussing a thing they do not know much about are either enthusiastic or mutter darkly and shake their heads. Well so do I. The risk that our fossil may be misplaced should always be before our minds when we deal with a thing too small to see; it exists even with good-sized specimens, at least in old collections.

I once tried dissolving some unfossiliferous things, bricks, slates and lumps of slag which had lain exposed on the moors. I cleaned them well and dissolved them, and in the residue there was pollen, in small amount, it is true; but, of course, Recent pollen.

Then I find it hard to understand the original form of squashed pollen grains. Often my wits are stretched to the limit when I deal with pollen from an intact fossil pollen sac and I know they were originally all the same. Dispersed pollen is surely more difficult; not too difficult (perish the thought), but more difficult than some people have realised.

But are we sure the Pre-Cretaceous Angiosperms or their barely Angiospermous ancestors look like Angiosperms? Perhaps not. I find this an encouraging thought, for it means that almost any of the less-known early fossils *might* be a candidate, and less-known fossil plants are enormously in the majority. There is nothing to be done but ordinary hard work, let it give what answer it will. I need follow this thought no further.

Then there is the question, where should we look for Pre-Cretaceous Angiosperms? Those who hold that when they invaded and dominated the Cretaceous scene they were already a great and highly evolved order, have no difficulty in finding a place off scene. Roughly speaking, nearly all the main plant-bearing deposits represent slowly sinking deltas and the vast majority of their fossil plants were either those of shallow pools, like reeds and mangroves today; or trees and herbs overhanging river banks. Anywhere else is more or less off scene because fossils of other places are seldom preserved, that is to say nine-tenths or more of the whole world.

It has been my luck to see something, albeit small glimpses, of floras that are not those of deltas. The well-known fossil flora of Yorkshire is the flora of a delta; but three times this delta was drowned by the sea and then was built up again. Shortly after it was drowned you find a very different flora; not the ferns, horsetails and Cycads of the usual flora; and not, I must say, anything I could see as a hint of an Angiosperm, but a whole lot of strange Conifers, particularly 'Monkey-puzzles'. Then in South Wales there are plant remains as I have mentioned, preserved in mud-filled fissures in limestone. The plants apparently grew on the dry limestone hill forming a heath vegetation or

perhaps the low scrub under a forest canopy, and then one day the vegetation caught fire and the charcoal was swept by the next rainstorm into cracks in the limestone. And the plants although they have value from their unusual preservation as charcoal fragments are every one of them ordinary plants of the period. No, if there were dominant Angiosperms we must look elsewhere. Not it would seem in any particular country, for with progress in palaeobotanical research they are telling the same story but to rather special environments like deserts or high mountains.

Professor Axelrod has put forward a hypothesis on these lines in which he commits himself to definite positive statements; just what a hypothesis should do. In the first place he is more generous than I in accepting rather obscure fossils as true Angiosperms and then he rejects the old suggestion that the Angiosperms swept on to the stage everywhere at about the same time. He has a perfect right to reject it. The fact is that far too many of the twenty or so floras which show a mixture of the old Gymnosperm orders and the new Angiosperms, are very poorly dated and their dates can be pushed up or down quite a long way without defying any proved fact. He arranges his data in a way that suggests that the change occurred fairly early in the Cretaceous in the tropics; and late in the Cretaceous in the Arctic and so occupied many million years altogether and not just a few. Then I think it is a good hypothesis because it tells us where to look for the real early Angiosperms—in the late Jurassic or Early Cretaceous in the tropics. But then comes doubt again. Where were the tropics in the Cretaceous and where was the Arctic? If the Greenland Cretaceous flora *was* Arctic, I can only say it does not look it. But that leads us to other important problems and we have enough on our plate.

Well what are we to do? I can only suggest a dull answer—to do our best with the material to hand, and in studying it let it show what it has to show and not to force it into an Angiosperm or any other mould. And there is material in plenty for those who are willing to work for it. Let me give you some figures.

I would say that the Jurassic flora of Yorkshire which has been studied scientifically for about 140 years must have had about as much time spent on it, in relation to its size, as any flora on earth. It is accessible and is in a sense well known. And it has been revised repeatedly; and no one has found a real Angiosperm in it: in its way it is the worst flora of its age to search for one, for it is about the best known. But it is by no means hopeless, a fossil is only hopeless as an ancestral Angiosperm when it is well enough known to be proved by its various organs to be different. Let me give you some figures. I will give you the progress of knowledge in the form of rough arithmetic; I will take first the total number of plant species, and that is species of leaves, and then the number of these species of which we know one other organ, a first grade and a second grade of knowledge. At the second grade the possibility that the plant might be an Angiosperm is virtually closed; it may have seemed highly unlikely at the first.

Now in 1900 my old professor and predecessor as President of Section K (Botany) in 1903, Sir Albert Seward, revised the Yorkshire flora, as others had done. He described a total of fifty leaf species of which twelve were related to a second organ, or, if you like, twenty-four per cent. Today with the job of revising the flora only half done I will limit myself to published work. I accept as valid 103 leaf-species, of which thirty-four are related to other organs. Now the percentage is thirty-three so in a sense we have gone very little forward. But it might be better to say that the species at the first stage are botanically only little better than a nuisance and those at the second worthy of respect; so the result of sixty years' work has been to add another twenty-two species to the ranks of the respectable. The figure looks better that way. But what a lot of room there is for the fossil botanist to have innocent fun in making progress among the sixty-nine now at their first stage.

If, in fact, you look at any account of a flora, such as the Yorkshire one you will find everything classified; but do not be discouraged, the classification is worth no more than the evidence on which it is based and that may be next to nothing. A few years ago I worked on the fructifications of one of the longest known and commonest leaves in the Yorkshire flora, *Solenites* or *Czekanowskia*. It has long been classified as an ally of the Ginkgo tree on evidence that was reasonable even if very slight. The fructifications proved utterly unlike that of Ginkgo; not, it is true, Angiosperm-like either but quite unexpected. The very numerous

species at the first grade of knowledge may show anything.

Nothing, I am afraid, will make a neat summary of this rambling address. I have no recipe for success, hard work may give what we want, but so far it has not. But where the work has been scholarly it has given its own reward by adding to knowledge some unexpected and strange plants. In fact, when I see a claim from someone to have an early Angiosperm, I have a feeling almost of compassion based on the statistics of earlier claims.

We want early Angiosperms urgently, almost as a thirsty man wants water. And like the thirsty man we are not critical about the quality of the water. Nor should we be. But then we should look again at the evidence as a well-dined man looks at water.

PRELIMINARY INVESTIGATIONS CONCERNING THE ORIGINS OF FOOD-PRODUCTION IN IRANIAN KURDISTAN

By Prof. ROBERT J. BRAIDWOOD *Dept. of Anthropology, University of Chicago*

The achievement of food-production and the village-farming community way of life was an event of paramount significance in the cultural history of man. Once plants and animals had been effectively domesticated, and food could be stored —on-the-hoof or in the grain-bin—vast new dimensions for cultural evolution became possible. The late V. Gordon Childe, in a number of thoughtful studies, considered the implications of this event, which he called the food-producing revolution. Without the achievement of food-production and the effective village-farming community way of life, the appearance of civilisation (in any meaningful sense) would not have been possible.

Several factors confront the prehistorian in the identification of the scene where this great event took place. Obviously, since the effective village-farming community way of life was the foundation upon which literate civilisations might subsequently arise, there is no chance of our ever finding contemporary written documents describing the event itself. The achievement of effective food production was made several thousand years before the appearance of writing. Here are some of the factors which the prehistorian must take into account in assessing and localising the beginnings of food-production:

1. Of what value are traditions (such as the Garden of Eden or other Biblical stories) or philosophical speculation (e.g. Hegel, Spengler, Toynbee, etc.) in locating the scene?

2. What is the present order of completeness of the archaeological record (both in terms of artifactual typology and of chronology), and of archaeologically oriented exploration?

3. What is the probable order of reliability in our interpretation of the available artifactua materials of the archaeological record?

4. Since the appearance of food-production can only have taken place in certain types of natural environments, what is the present order of knowledge of palaeo-environments of the approximately pertinent areas and ranges of time?

5. How many independent instances of the achievement of effective food-production may there have been, and what other instances may have depended on the effects of diffusion from any one original scene?

Thus we could not—at this writing—categorically deny that the earliest instance of the achievement of food-production took place, for example, along the northern slopes of the Hindu Kush. We do not know enough to make such a categorical denial; we could only say that the presently available evidence suggests this would have been an unlikely locale.

The Iranian Prehistoric Project, with essentially the same senior staff as that of the old Iraq-Jarmo Project,[1] has undertaken its first field season (September 1959 to June 1960) in the environs of Kermanshah, in western Iran. The problem orientation of the project is one of reclaiming and examining evidence for the incipience and earliest development of a food-producing village-farming community way of life. Our choice of the Kermanshah area reflects our theoretical conviction (with respect to the factors listed above) that the western slopes of the Zagros mountains were part of a locale of primary importance for one instance of the incipience and earliest development of food-production. In fact, we take this South-west Asiatic instance to have been the world's earliest

example of the achievement of food-production, although we believe there were subsequent independent instances elsewhere (for example in Mesoamerica).

Our conviction as to the whereabouts of the South-west Asiatic nuclear or natural habitat zone for the incipience and early development of food production depends on an increasing attention to the problems of human palaeo-environments by natural scientists. There are recent summaries of the matter.[2] In brief, it now appears that the zone from *ca.* 1500 to 4500 ft. in elevation, which arcs about the headwaters and tributaries of the Tigris and Euphrates rivers, did contain the potential plant and animal domesticates some ten thousand years ago. However imprecise a present-day ecological delineation of the zone may be, we believe its arc-like extent (from the region of Shiraz in south-central Persia to the Cilician highlands beyond Gazi Aintab in Turkey) must have covered at least some 1200 miles.

The degree to which all of the potential domesticates were also at home along the lush Mediterranean littoral of the Levant, or behind the rain-shadow of the Anti-Lebanon and its extensions, is in some dispute. There is a further question: even if some of the potential domesticates inhabited these regions, at what moment in time did their domestication become culturally effective? A botanical rechecking of older reports that *Triticum dicoccoides* (wild Emmer wheat) grows at low altitudes may be critical in this matter. Another critical point would be an ecologically oriented assessment of the theory that, with a *supposed* increase in the desiccation of the area at the end of the last glaciation, the potential domesticates retreated *downward* to oases or riverine habitats.[3] Such things are at issue, as well as the relative reliability of various radiocarbon counters, and the degree to which certain samples may have suffered 'geobiochemical' contamination and thus yield false age determination, in the so-called Jarmo-Jericho controversy.[4] Our own selection of still another portion of the Zagros flanks zone (some 120 miles south-east of the Jarmo area in Iraqi Kurdistan), for further investigations of the earliest effective village-farmers, emphasises our present conviction concerning the promise of this part of the locale.

Arriving newly in the Kermanshah area, whose prehistory was almost completely uncharted, we spent the first three months of our present field season in surface survey. The valley-plain of Shahabad and fair portions of the valley-plains of Mahidasht and Kermanshah have been covered, and our maps now show over two hundred prehistoric mounds, open hill-top scatters and caves. We did not feel it within our competence or purpose to deal with the surface yields of many more sites of the historic periods. The surface survey will be resumed before the season closes, but by January 15, 1960, we began short series of test excavations on sites of particular promise for our central problem. By the beginning of March, our applications for formal permission to excavate were cleared by the co-operative Iranian authorities.

As the surface survey progressed, we were able to make a tentative classification of the surface yield of potsherds and flint tools. This classification consisted of a subdivision of these materials into eight models, which we took to represent the traces of different periods of prehistoric cultural activity. The selection of the surface materials for each model was done (1) deductively, by noting the clusters of characteristic flint and/or pottery groups which appeared either alone (on apparently one-period sites) or which, in combination (on sites with several periods of occupation) stood out as units distinctive from one another and from obviously later wheel-made or glazed ceramics; and (2) comparatively, in terms of either our own experience with the prehistoric materials of Iraq, or through reference to the standard treatments of Iranian prehistory[5] and such excavation reports as we had at hand. These surface survey models and their order will clearly need some revision and —certainly—subdivision, as knowledge is increased through actual excavations. A rough description of these models and their suggested chronological positions is as follows:

1. Flint hand-axes of standard Acheulean type, with associated flake tools, from surface hill-top scatters. *Ca.* 100,000–75,000 years ago.

2. Flint tools of Mousterian type in perhaps two facies from both open-air and rock-shelter occurrences. *Ca.* 75,000–35,000 years ago.

3. Flint tools of various types pertaining to the blade-tool preparation tradition, in perhaps two major aspects, with those generally similar to the

Zarzian industry predominating. *Ca.* 35,000–10,500 years ago.

4. Flint tools of a generalised late blade-tool tradition (with a microlithic complement continued), from open-air encampment sites, along with some indications of intensified collecting activities (clam shells, etc.) and of simple ground stone and clay artifacts. Contemporaneity—although hardly exact technological comparability—with the Karim Shahirian[6] of Iraqi-Kurdistan is suggested. *Ca.* 11,000–9,000 years ago. (We consciously suggest the slight temporal overlap between models 3 and 4.)

5. Flint and obsidian tools of a blade and microblade industry, ground stone bowl and bracelet fragments and figurine fragments highly suggestive of the Jarmoan[7] phase in Iraqi-Kurdistan. Of the pottery in this model, some painted pieces are suggestive of the Jarmoan, but red-burnished ware, other painted sherds and a peculiar slip-paint (a thin buff clay-like solution used as a paint on black or red-burnished surfaces) are not seen at Jarmo. *Ca.* 9,000–8,000 years ago.

6. Sherds of a well-developed and varied ceramic industry comprised of the following elements: (*a*) some generalised but few specific painted motifs might be Halafian; (*b*) a red-surfaced series which may carry simple decoration with black paint or white paint or both; (*c*) a series with rather glaze-like 'Urfirnis' surfaces; (*d*) a red slipped and burnished series with certain characteristic profiles, and (*e*) a coarse simple-ware series with a few profiles vaguely recalling those of the Hassunan. Some of these wares and traces of their associated flint industry also appeared in surface collections we made at Tepe Giyan, and must refer to the basal levels of that site. *Ca.* 8,000–7,000 years ago.

7. Sherds with a considerable variety of the painted motifs of McCown's 'buff-ware' type (although perhaps more in the repertoires seen at Bakun A or in the Ubaidian than in classic Susa I). This model must include several phases of development within the general 'buff ware' tradition. Large and symmetrical flint blade-cores and blades. *Ca.* 7,000–5,750 years ago.

8. Sherds of the simpler Uruk–Proto-Literate series; red and grey wares, simple wares with drooping spouts, or small pierced-lug handles, or the bevelled-rim bowl (cf. Le Breton's Susa B-C groups). Flint blades which closely approach those of the 'Canaanean' type. *Ca.* 5,750–5,000 (+ ?) years ago.

We would insist that these working models suggest only gross and unrefined subdivisions of both time and prehistoric cultural activity. This has already become clear as our activities have progressed from surface survey to *sondages* and formal excavation.

Excavations are now completed at the rock-shelter of Warwasi, in the throat of the Tang-i-Kenesht valley, 12 km. north-east of Kermanshah. The stratification runs from at least one phase of the Mousterian industry (cf. model 2) through sequence of blade tools (cf. model 3) from Baradostian[8] into Zarzian types. Warwasi enlarges knowledge of the types of materials recovered in the rock-shelter at Bisutun,[9] and may—when the processing of its yield is complete—suggest something of the course of typological development between the Barodostian and Zarzian industries. This range has heretofore not been accounted for.[10] Our model 3 may thus call for subdivision.

The open settlement site of Tepe Asiab, 6 km. east of Kermanshah, has—at this writing—only been investigated by sondage (its formal excavation will be undertaken within a fortnight.) Our soundings suggest that the site was once an encampment of intensified food-collectors (of model 4 type) who utilised quantities of clams (*unio*) from the Kara Su river, which flows by the base of the hill on which lies the site. The Asiab flint industry is not strictly Karim Shahirian in type, being appreciably more developed. The soundings also yielded items in ground stone and numerous small clay objects including a few fragments of enigmatic figurines. A large series of coprolites appeared. Should these prove on analysis to be human, they will form an invaluable clue to the diet of the Asiab people.

Tepe Sarab, a small and low mound 7 km. east-north-east of Kermanshah, contains the remains of a simple village of a phase within the time range of model 5. At this writing, the exact type of its architecture is unclear, but the site may contain the remains of pit dwellings. The preponderant pottery is a burnished ware, about half of which is red surfaced. There is also a painted series, of which the simpler decoration recalls that of Jarmo, but the Sarab series runs to much more

complicated brocaded motifs. These motifs may suggest more southerly Iranian contacts; it seems to us that they resemble motifs illustrated from Djari B and Mushki.[11] There is relatively little of the slip-painted series, noted as part of the general model 5 content, on Sarab.

Both the Sarab figurine and flint and obsidian categories strongly recall those of Jarmo, as do the ground stone bowls and bracelets. In our own overall assessment of the two assemblages (Jarmo vs. Sarab), we would suggest that of Sarab to be the slightly more developed and varied one. It does not necessarily follow that Sarab was later than Jarmo, chronologically.

Several sites in our surface survey were similar to Warwasi, Asiab and Sarab. We chose these particular sites for excavation partly because of the promise of their surface yields, but in fair part also simply because of their convenience to our base camp.

We are not yet in a position to say more of the cultural developments in the Kermanshah environs after Tepe Sarab, beyond what is already suggested in our sequence of models. It does not appear to us—against our Iraqi background of experience—that there were direct linkages between either the Hassunan (and Samarran) or the Halafian assemblages and anything we have yet seen in the Kermanshah region. It would rather appear that, following the incipience and achievement of the village-farming community way of life (along the Zagros flanks natural habitat zone) during the time range of our models 4 and 5, this part of Iran then proceeded in the direction of a cultural development which was regionally its own. Only at some time within the duration of model 7 did this part of Iran, at least, again become a part of the broad generalised *oikoumenē* of the Ubaidian phase in South-western Asia. It is yet to be discovered what may have been this region's role in the formation of certain characteristic cultural elements of the generalised Ubaidian phase.

It will, of course, require many years and much field research—by prehistorians and natural scientists working together—before the delineation of the locale for the earliest achievement of food-production in South-western Asia even approaches completeness. For example, whenever the interpretation of the Turkish antiquities law makes modern problem-oriented research feasible, much evidence pertinent to the whole matter is bound to be recovered in the upper basins of the Tigris and Euphrates. As things now stand, it is completely possible that our own theoretical convictions concerning the localisation of the scene are in error. Our assessment of the factors bearing on this localisation (suggested earlier in this article) may themselves be in error.

One thing does become increasingly clear, however. Our colleagues in the natural sciences continually remind us that, at its core, our problem is a cultural one. Plants and animals did not domesticate themselves; man was the domesticator. Our natural scientist colleagues thus warn us against snatching at facile assumptions based on theories of environmental determinism. Problems in human history must be approached with due respect to the factors of cultural evolution itself. With the techniques now available, there are probably more immediate results to be gained in working to reduce the imponderables of the bearing of environments on cultural evolution. We should not, however, delude ourselves into believing that this will complete the task.

References

1. As was the Iraq–Jarmo Project (cf. Robert J. Braidwood and Bruce Howe, *et al.*, *Prehistoric Investigations in Iraqi Kurdistan*, Chicago, 1960), the Iranian Prehistoric Project is a joint venture of the Oriental Institute of the University of Chicago and the Baghdad School of the American Schools of Oriental Research. In Iran, we have been joined by the Institute of Archaeology of the University of Tehran. Both projects have received aid from the National Science Foundation (for the participation of a team of natural scientists), from the Department of Anthropology of the University of Chicago, the Penrose Fund of the American Philosophical Society and the Wenner-Gren Foundation for Anthropological Research. In Iran we have also had support from the U.S. Public Health Service, the Khuzestan Development Service and the National Iranian Oil Company, and the ready co-operation of the Iranian Government's Antiquities Service. Robert J. Braidwood serves as general director of the project for the University of Chicago, Bruce Howe (of the Peabody Museum, Harvard University) as associate director represents the American Schools, and Ezat O. Negahban serves for the University of Tehran.
2. In Robert J. Braidwood and Bruce Howe *et al.*, *op cit.*; Hans Helbaek, *Science*, **130**, 365 (1959); Charles A. Reed, *Science*, **130**, 1629 (1959).

3. Cf. CHARLES A. REED, *loc. cit.*
4. Cf. KATHLEEN M. KENYON, *Journ. Roy. Anthrop. Inst.* **89**, pt. i, 35 (1959), and ROBERT J. BRAIDWOOD, *Österr. Akad. Wiss., Phil.-Hist. Kl., Anzeiger, Jahrg.* 1958, **19**, 249 (1959). In brief Dr. Kenyon interprets the archaeological materials from the sequence of her basal layers in the Dead Sea valley site of Tell es-Sultan (traditionally identified as the site of the Biblical Jericho) as evidence of 'an independent sequence in Palestine'. Some of my colleagues and I are unable to agree with her reasoning, and are unconvinced of the necessity for postulating more than one generalised sequence towards the establishment of an effective village-farming community way of life in South-western Asia.
5. E.g. LOUIS LE BRETON, *Iraq*, **19**, 79 (1957); DONALD E. MCCOWN, *The Comparative Stratigraphy of Early Iran*, Chicago, 1942; LOUIS VANDEN BERGHE, *Archéologie de l'Iran Ancien*, Leiden, 1959. Brief descriptions of most of the prehistoric Iraqi assemblages (e.g. Hassunan, Halafian, etc.) may be found in ROBERT J. BRAIDWOOD, *The Near East and the Foundations for Civilization*, Eugene (Oregon) 1952.
6. ROBERT J. BRAIDWOOD and BRUCE HOWE *et al*, *op. cit.*, n. 1.
7. *Op. cit.*
8. RALPH S. SOLECKI, *Smithson. Inst. Publs. Rept.* no. **4190**, 389 (1954), Washington, 1955.
9. CARLETON S. COON, *Cave Explorations in Iran* 1949, Philadelphia, 1951.
10. ROBERT J. BRAIDWOOD and BRUCE HOWE *et al.*, *op. cit.*
11. VANDEN BERGHE, *op. cit.*, 41–2, pl. 49.

SIXTY YEARS OF PROGRESS IN ENGLISH EDUCATION*

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Periods of radical social change tend to be intensely frustrating to people intent on promoting change. Conscious that opportunity is at hand, and more keenly aware than their fellows of defects and inadequacies in the social order, nothing but sudden and wholesale reform can satisfy them. They cannot tolerate the idea of the 'inevitability of gradualness', however much this may seem to others to be a law of nature—at least in the Western world of free democracy and mature civilisation. Such frustration often leads to a most unhappy misconception of the contemporary situation; the feeling that so little is being done induces the belief that things were never worse. This belief is totally fallacious, but very common: and never more so than at present. The only effective cure I know is to look back over the past, and to endeavour to see the present in historical perspective. That is what I propose to do in this paper.

I have chosen the period of sixty years for my survey for two reasons, one logical, the other largely sentimental. On April 1, 1900, all public education in England and Wales was, for the first time in our history, placed under the supervision of a single government department: the Board of Education. And, at some later date in that year (almost certainly in early September), I began to attend school: a Public Elementary School. From that day to this, except in the years 1914 to 1919, I have been professionally concerned with English education: as a pupil at school up to 1910, and thereafter in various capacities: teacher in primary and secondary schools, youth leader, educational journalist and author, and university officer. I can speak, therefore, with knowledge reinforced by personal experience over almost the entire period.

* * *

In 1900 England was educationally a sadly underdeveloped country, with a statutory system of public education, by present-day standards, almost unbelievably primitive. May I remind you of a few of its grimmer facts? Of the 5,700,000 children on the rolls of the Public Elementary School over 5,400,000 were under the age of 13: and many thousands of those had to divide their day between school and employment. Children could be employed part-time in agriculture from the age of 11, and part-time and even full-time in that and other occupations from the age of 12. A recent Minister of Education, the late George Tomlinson, became in 1902, at the age of 12, a 'half-timer' in a Lancashire cotton mill, which meant that he worked there, on six days in the week, for a total of 30 hours, and also attended school for five half-days. This was called 'beneficial employment'. There were at the time nearly 90,000 of such little 'part-timers', most of them in Lancashire or Yorkshire. I sat alongside of some of them in a West Riding elementary school.

In 1903, at the age of 13, George Tomlinson left school, and became a full-time employee, working 56½ hours a week. Leaving school at 13 was still widespread, despite the fact that an Elementary Education Act passed in 1900 had empowered local authorities to keep children at school till 14. Such bye-laws as were made were often openly flouted. An H.M.I., reporting to the Board of Education in 1901, wrote despairingly that:

> Education Acts may fix 14 as the age at which legal obligation to attend school ceases, managers and teachers may do their best to uphold the law, but

* Address delivered to Section L (Education) on September 2, 1960, at the Cardiff Meeting of the British Association.

I can't honestly say that I ever came across a case in which, if a child chose to leave at 13, in the country at all events, anyone has succeeded in preventing the departure. I do not think the experiment is often tried . . .

Children not only left school early; they also often started late. Another H.M.I. wrote, in the same year:

In the infant schools I have been struck with the number of old and backward children who begin to attend school so late that they can only receive a hurried and superficial preparation for what is really premature transference to the school for older scholars; and in the upper schools with the presence (almost invariable in the larger ones) of a 'Standard 0', consisting partly of some of these cases, partly of children of 8 to 12 or even 13, some of whom may never have been to school before, while the attendance of others has been so sporadic and casual that it has been of little use.

Irregular attendance was rife. It was frequently ignored, and sometimes connived at, by persons in positions of responsibility.

A rural district [wrote an H.M.I.] provides unusual opportunities for the habitual irregularity of both boys and girls, which is encouraged by the no secret knowledge that the authorities will take no notice of children who attend about seven or eight of the ten weekly meetings.

Another H.M.I. recorded that:

At a certain country railway station, boys regularly congregate for the purpose of attending on golfers who arrive by train. . . . I complained to the village schoolmaster. 'Would it not,' I asked, 'be possible to prosecute one of these gentlemen?' The answer I got was significant: 'He might be a J.P.'

A very great deal of poor attendance, however, was caused by ill-health, though every day children could be seen in school who would now be in bed at home, if not in hospital. In 1900 there was neither School Health nor School Meals Service, and the only statutory provision for handicapped children was for the blind and the deaf. A few School Boards were voluntarily providing a modicum of medical attention, and some care for the handicapped. Voluntary organisations were providing meals for a minute proportion of the vast number of undernourished children. But that was all.

Disease, physical defect, and malnutrition were rampant. Over three-quarters of all the children in public elementary schools had badly decayed teeth. Adenoids, infected tonsils, middle-ear disease, ringworm, rheumatism (called 'growing pains'), and rickets were so common as to pass without comment. Infestation was almost universal.

* * *

In charge of the 5,700,000 children on the roll of the Public Elementary School there were some 143,500 teachers: roughly, one to forty children. But of these teachers nearly 80,000—well over half—were unqualified: 32,500 uncertificated, 17,500 'additional women teachers' (i.e., 18 and vaccinated), and 29,500 pupil teachers, that is, children between the ages of 13 and 18 who were serving their apprenticeship, teaching during the school day and studying before and after and at the weekends. There was only one certificated teacher for every 89 children on roll. Mercifully, only about 73 of these were in attendance on any given day. Being certificated did not necessarily mean that the teacher had been to training college; in 1900 over a quarter of the men, and over half of the women who held teachers' certificates had not. They had served their time as pupil teachers and duly passed the required examinations. This was because Training College accommodation was so grossly inadequate that each year a large majority of the qualified applicants failed to secure places. Recruitment was from successful candidates in the annual Queen's Scholarship examination, open to pupil teachers on completion of their apprenticeship. In December 1899, 12,120 candidates entered, and 10,128 passed; but only 2,732 of these were admitted into college. A man who passed the examination had about a one in three chance of being accepted; a woman hardly one in five.

Of the curriculum and teaching methods of the Public Elementary School at the beginning of the twentieth century I could speak at length, with much feeling. I suffered them for four years only, and was exceptionally fortunate in one of my teachers; but I sometimes feel that I have never fully recovered from them.

The curriculum was meagre, the instruction arid, the discipline usually Draconian. Here are some sample glimpses. To learn to read we first mastered the alphabet, letter by letter. Then we spelled out strings of single-syllable words having a common vowel and consonant—C–A–T, cat, B–A–T, bat, M–A–T—later building up these words into such

exciting sentences as 'The cat sat on the mat'. The process was repeated with two-syllable, three-syllable, and polysyllable words until we were judged to have a sufficient vocabulary to tackle a 'Reader'. My 'Readers' (we had only one a year) consisted chiefly of pseudo-historical and geographical anecdotes: Alfred and the cakes, and so on. That was our sole excursion into literature.

In writing we began on slates and advanced to copybooks; in the latter we strove laboriously to approach—we could never hope to equal—the inhuman perfection ('up strokes thin, down strokes thick') of the model line that flowed across the head of each page, reminding us with all the force of its innumerable sinuous curves that, for example, 'Too many cooks spoil the broth', or that 'A stitch in time saves nine'.

I forget how we were introduced to figures, but 'Sums' I shall never forget. This began after weeks and months of chanting in unison the multiplication tables. It opened with 'mental'—a prodigious exercise of calculation assisted by imagination and chicanery. The teacher quick-fired questions like 'Eight plus nine minus seven plus thirteen?', and 'Sixty-five loaves at tuppence-ha'penny?' The class strove to quick-fire back answers, hoping against hope that occasionally one would be correct. Fingers were forbidden as computers, but we all used them behind our backs. The next, and final, stage was the working, on slate or paper, of endless series of sums in all the known weights and measures, and either starting or ending with amounts running into millions.

In geography and history we plumbed the very depths of banality. Geography consisted of memorising, in their order round the coast, the capes, bays, estuaries and rivers of England and Wales: 'Dungeness, Beachy Head, Selsey Bill . . . Witham, Welland, Nen . . .' and then the mountain ranges, the counties and their capitals. In history we similarly memorised, in chronological order, the names of the kings and queens of England, aided on our way by a jingle that began:

> William the Conqueror long did reign,
> William his son by an arrow was slain . . .

My schoolmates and I used to call the victor of Hastings the 'corn curer'—but not in the teacher's hearing. Pupils did not *speak* to teachers in those days; they only answered their questions. Any other form of conversation in class was quickly cut short by the cane, then the ubiquitous panacea for all errors and delinquencies.

Music was represented by the singing (a courtesy title) of the tonic sol fah scales; I cannot recollect that we ever got as far as a melody. The only instruction in art I ever received was in how to paint 'blobs': oval impressions in water-colour placed, like the spokes of a wheel, round a central point. The result was called a flower. My sole exercise in 'handicraft' was the making of a housewife's workbasket with green rushes.

Perhaps I was unlucky. The Board of Education's report for 1900–1 assures me that grants were paid for drawing in respect of nearly 2 million pupils, that manual instruction was taught in 1,708 schools (out of 20,117), and physical exercises in 9,675. But though I attended three Public Elementary Schools in widely different parts of the country between 1900 and 1904 I never received instruction in any of these. Nor did I see anything of the cookery taught, so the Report declares, to 20,000 girls.

* * *

There was no statutory system of secondary education. The so-called Schools of Science and Art—frequently no more than classes of older pupils in elementary schools—gave a partial and rudimentary form of secondary instruction. A few endowed grammar schools offered scholarships to elementary school pupils: that was how the late Sir Ernest Barker, son of a Lancashire labourer, was launched upon his distinguished academic career. But Mr. G. A. N. Lowndes has estimated that in 1895 not more than three to six elementary school children in 1,000 entered the grammar school by this means: and I doubt if the proportion was much higher in 1900. When, in 1904—two years after a statutory system had been established, I told my classmates in a Yorkshire elementary school that I was going to a secondary school they stared at me in bewildered amazement. They had never heard of such a thing before, and they could not understand why anyone could wish for more schooling. When I recall the deplorably low standards of work, discipline, and behaviour at the private grammar school which I attended for a year I am tempted to think that they were more fortunate than I. Certainly, if any of them were later stimulated to seek further

education, they got far more useful and competent instruction in one of the evening continuation schools provided by the local authority. In 'night' school at that time the subjects 'more taught than any other' to the half-million scholars in attendance were reading, writing and arithmetic. And able and ambitious students could go on from continuation school to one of the new Technical Institutes that had begun to spring up in numbers in the 1890s.

University education, except at Oxford and Cambridge, then perhaps at the highest peak of excellence they had ever achieved, was much of it in almost as rudimentary a state as elementary, secondary, or technical education. Of the four modern universities—Durham, London, Manchester, and Wales—and seven university colleges, most were only partially occupied in work of university standard. London only became a teaching university in 1900, after sixty-four years as an examining body. At most of the university colleges students reading for degrees formed a tiny minority: of Birmingham's 677 students in 1901 (a year after university status had been attained) only 29 were awarded first degrees, and only 9 higher degrees. In the five years 1896–1901 the University Colleges of Nottingham and Sheffield together secured only 46 degrees. At some of the colleges evening students were far more numerous than day: at the Durham College of Science 1,000 to 475, at Sheffield, 1,080 to 340, at Nottingham 1,660 to 430. And many of the daytime students were members of nearby teacher training colleges.

* * *

It is difficult to realise that this state of affairs represented a marked improvement on that of immediately preceding years. Yet all the accounts testify that it did. The fresh breezes of reform had begun to blow strongly in the 1890s. In the first decade of the twentieth century they rose to gale force.

Recently a Minister of the Crown described as 'explosive' the growth in English education since the passing of the 1944 Education Act. The same epithet can justly be applied to the growth which took place between 1900 and 1914. In both cases much combustible material had been laid beforehand. For example, in 1889 annual State subsidies to university institutions became established, and the Technical Instruction Act sanctioned local authority subsidies to technical colleges. In 1894 the Bryce Commission's report made the early creation of a statutory system of secondary education inevitable. In 1897 the last remaining fetters of the horrible system of 'Payment by Results' were struck from the wrists of elementary school teachers—though not, alas, from their minds. In 1900 a new Code of Regulations for Public Elementary Schools officially confirmed the reality of this freedom.

> For the first time for 40 years, wrote an H.M.I., teachers will be free in arranging their timetables to consider the educational worth rather than the supposed money value of a subject; for the first time the Code draws up a curriculum which lays down the foundation of a 'liberal education'.

Five years later Sir Robert Morant, first Permanent Secretary of the Board of Education, in his preface to the first edition of the *Handbook of Suggestions for the Consideration* (significant words!) *of Teachers and others engaged in the Work of Public Elementary Schools*, was officially to extend this freedom into the realm of method as well.

> The only uniformity of practice that the Board of Education desire to see in the teaching of Public Elementary Schools is that each teacher shall think for himself, and work out for himself such methods of teaching as may use his powers to the best advantage and be best suited to the particular needs and conditions of the school.

We are today justly proud that English teachers have, as teachers, more freedom than any others. It is perhaps wise to recall occasionally that this freedom is a plant of recent growth.

* * *

The Education Act, 1902, was described at the time of its passing as 'probably the most important event in the history of education in England since the full recognition of Elementary Education as a national duty in 1870'. This was no exaggeration. The Act effected three reforms of fundamental importance. It transferred the local control of public education from *ad hoc* bodies (nearly 17,000 of them) to general purpose local authorities, and thus made public education a public responsibility in the fullest sense of the term. It gave to the major authorities, the County and County Borough Councils (which between them covered the whole of England and Wales), the power to supply or aid

'education other than elementary', and so brought all forms of public education, in the localities as at the centre, under a single authority—though unfortunately not everywhere completely, since public pressures compelled the creation of 'Part III' authorities, responsible for elementary education only. It vested the control of secular instruction in voluntary schools in the public authorities, in return making the maintenance of these schools a charge upon local rates as well as national taxes; and thus enabled these schools—at the time far more numerous than the 'Board schools'—to continue to play a large part in the national provision.

The power given to the County and County Borough Councils to supply and aid education other than elementary made possible (as was intended) the creation of a statutory system of secondary education. Both the Board of Education and the L.E.A.s for years devoted their main effort to building up this system. With great success; its growth was spectacularly rapid. By 1905 there were over 700 recognised secondary schools, and by the outbreak of war in 1914 over 1,000, containing 187,000 pupils—more than six times as many as were estimated to be receiving efficient secondary education in 1900. This, remember, on the basis of permissive powers only; the 1902 Act laid upon the L.E.A.s no statutory *duty* in respect of secondary education.

Nor was the growth merely numerical; secondary education was made available, if only on a competitive basis, to all, irrespective of means. Secondary schools remained fee-paying schools, but in 1905 the Board of Education increased the number of its own scholarships, and urged L.E.A.s to do the same, in order to get into the schools 'the cleverest pupils . . . without distinction'. Two years later the Board, under the Education (Administrative Provisions) Act, 1907, offered substantially larger *per capita* grants to secondary schools which would admit a proportion of entrants—normally equal to 25 per cent of the previous year's entry—on 'free places' awarded to public elementary school pupils.

Increased quantity of provision is, however, neither the only nor the most important criterion by which to assess progress; nor even is extension of opportunity. One must ask of what the opportunity consists. Many people have thought that our statutory system of secondary education was started off on too narrow a curricular basis. I cannot here elaborate on this highly controversial matter; the argument is set out in the historical chapter of the 'Spens' Report. What is certain is that while the Elementary School teacher was, officially, granted virtually complete freedom in respect of curriculum and method, the secondary school teacher was not. The 1904 Regulations—the first issued for secondary schools—put him firmly into a straitjacket which closely resembled that worn by the endowed and public grammar schools. I do not defend this; but it is pertinent to recall that:

> The reports on Secondary Schools in several parts of the country made by some of the Board's Inspectors and issued in 1903, gave grave cause for anxiety as to the teaching in those schools. There was evidence of too early a specialisation, there was a gradual loss of the cultivation of language and literature, and a tendency to substitute a too exclusively scientific for a truly liberal education.

Those words might have been written today. And we are advocating exactly the same remedy that Morant applied: a good general education, defined in not widely dissimilar terms.

The same Act of Parliament which introduced the 'free place' system also quietly initiated one of the most beneficial services of the century. It gave statutory sanction to the recommendation made in 1904 by the Inter-Departmental Committee on Physical Deterioration that 'a systematised medical inspection of school children should be imposed as a public duty on every School Authority.' The School Medical Service thus launched was within a generation to transform the health and physical condition of the children of England and Wales.

In the previous year another Act for the benefit of children's health had been passed, the Education (Provision of Meals) Act, 1906. This empowered the L.E.A.s to spend money on meals for elementary school pupils deemed 'unable by reason of lack of food to take full advantage of the education provided for them'. But the Act was permissive only; it imposed no duty. The unfortunate result was that while the statutorily required School Medical Service went from strength to strength, the School Meals Service, sustained only by statutory permission, remained until 1941, a niggardly provision.

Until 1905 the training of teachers (other than pupil teachers) was undertaken entirely by voluntary bodies: in residential training colleges largely

by religious denominations, and in day training colleges (from 1890) by universities and university colleges. In 1904 the Board of Education, by offering to L.E.A.s a 75 per cent grant for building training colleges, induced several to enter this field. Within five years they had opened a dozen colleges, and within ten the annual output of college trained teachers had been doubled.

Between 1900 and 1909 five university charters were granted in England: more than in all the previous centuries of English history. The university colleges which received them had still a long way to go; but they had earned their promotion. As early as 1901 the Government's inspectors had declared of them (and others of the colleges) that 'the advance in the standard of work is more striking than the advance in (student) numbers'. Similar tributes were paid in later years. But there was also a national need for more universities; in particular, the new secondary schools needed an ever-increasing supply of graduate teachers. The Board of Education helped by offering university grants in return for a pledge to teach—and the intending teachers raised the standards of work in the new universities, and, not unimportantly, kept their Arts faculties alive.

Least substantial progress was made during these years in the field of technical education. Yet even here there was some gain; in particular, two developments cannot be overlooked. In 1904 the Board of Education greatly widened the range of subjects that could be taught in 'night school'. In 1905 the Board recognised for grant, and in 1913 dignified with Regulations of their own, the day technical classes which had been started in London and elsewhere for pupils from public elementary schools. Those classes are the direct ancestors of the Technical Secondary School; and even the College of Advanced Technology owes much to them.

Outside the statutory system there occurred in 1903 an event of far-reaching importance when Albert Mansbridge brought the universities, the Co-operative movement and the trades unions together into the partnership which produced the Workers' Educational Association. In 1908 R. H. Tawney began to tutor the first three-year tutorial classes, and thus launched the W.E.A's most outstanding contribution to adult education.

Here we reach the realm of imponderables. The influence of the W.E.A. can never be assessed. Nor can that of much else which happened between 1900 and 1914, a period singularly rich in events and ideas that have had beneficial effect on English education. For example, in 1902 Margaret and Rachel McMillan put before the L.C.C. a plan for 'a health centre—a place of healing in schools'; in 1905 Margaret published *Education through the Imagination*, an epoch-making book; in 1908 the two sisters opened the first school clinic, and in 1911 the first open-air Nursery School. In 1905 Alfred Binet published the first workable intelligence tests, and in 1913 the L.C.C. appointed the first educational psychologist. In 1907 Maria Montessori opened her first *Casa dei Bambini* and Baden-Powell held his first Boy Scout camp. A long list of other people could be given whose ideas and actions began to enrich English education between 1900 and 1914; it would include such diverse personalities as John Dewey, Miss Catharine Dodds, Sir John Adams, F. H. Hayward, Otto Salomon, Homer Lane, Edmond Holmes, Norman MacMunn and Caldwell Cook.

* * *

The First World War checked material progress, but, after its initial numbing shock, stimulated the flow of ideas. The war-time increase in juvenile delinquency caused widespread public concern, as did also the ruthless exploitation of young people in employment. The latter caused the setting up in 1916 of the Lewis Committee, which in its Report asked:

> Can the age of adolescence be brought out of the purview of economic exploitation and into that of the social conscience? Can the conception of the juvenile as primarily a little wage-earner be replaced by the conception of the juvenile as primarily the workman and the citizen in training?

The subsequent legislative attempt to realise that novel conception was a resounding failure—which we have not yet retrieved. But though the day continuation school clauses of the 'Fisher' Education Act of 1918 proved abortive, other reforms it enacted have proved of lasting benefit. It empowered L.E.A.s to provide nursery schools (not that many did), abolished fees in public elementary schools, required L.E.A.s to provide advanced and practical instruction for their older and abler pupils, restricted the employment of children before and

after school and, by fixing the school-leaving age at 14 for all, without exception, ended the pitiful tragedy of the 'little part-timer'.

* * *

The story of English education between the wars is far from being a happy one; it reflected all too faithfully the country's economic plight. But it had its great moments, especially in the realm of ideas.

One should not forget that it opened with a remarkable burst of public enthusiasm for education, which has been, I think, too little stressed by historians; they have, perhaps, concentrated too much attention on the slashing cuts ('Geddes Axe' and all that) inflicted on the public system of education in the alleged interests of the national economy. May I briefly illustrate the other aspect?

In 1919 the pressure on the secondary schools was such that some 20,000 applicants, fee-payers as well as free-placers, had to be denied admission because of lack of room; and this despite the fact that there were by then 1,200 maintained and aided schools with places for nearly 340,000 children—nearly double the number in 1914. The universities and university colleges were subjected to similar pressure. Unlike the schools, their grants were not cut; on the contrary, these were raised spectacularly. Even so, they lagged behind student numbers, which by 1925 had more than doubled the 1914 figure, and which, with but slight fluctuations, remained at that height up to 1939.

One of the greatest educational Reports ever written, that of the Adult Education Committee of the Ministry of Reconstruction, published in 1919, should have called forth the same measure of financial support for liberal studies for adults. But, alas, the value of liberal adult education was insufficiently apparent to the politicians, and the only substantial response to the Report came from the universities, which greatly expanded their extramural services.

The needs of adults were ignored. Those of the nation's children could not be. The continuing pressure upon the secondary schools forced attention to the many able children who failed to secure admission to them. The problem was referred to the Consultative Committee of the Board of Education—the suggestion came, the late Sir Ernest Barker told me, from an elementary school teacher. The result was one of the most seminal documents ever published in this country: the Committee's 1926 Report on *The Education of the Adolescent*.

It is difficult today to convey any idea of the thrill which ran through the educational world on the publication of the 'Hadow' Report. Time has outdated some of its arguments and conclusions, but can never dim the lustre of its massive achievement. It sounded the death knell of the idea that the nation's educational system should be organised along the lines of social and economic division, and established the idea that it should be organised on purely educational principles, in progressive stages freely and equally available to all, and providing increasing variety of content to match the increasing variety of ability, aptitude and interests which growing maturity brings.

Unhappily, ideas are rarely translated swiftly into satisfying action; and the late 1920s and the early 1930s, harassed as they were by world-wide economic depression and vast and persisting unemployment, were singularly unpropitious for rapid educational advance. Nevertheless, 'Hadow reorganisation' got under way, and as it did many primary school teachers, aided by two further brilliant 'Hadow' Reports—on *The Primary School* and *Infant and Nursery Schools*—began to understand the meaning and importance of primary education conceived as the first and foundation stage of a lifelong process, while their colleagues in senior elementary and central schools acquired at least some insight into the problems of giving secondary education to children of modest intellectual ability and non-academic interests. But it took eighteen years and the impact of war to get even the legislative proposals of the first Hadow Report implemented; and its fundamental ideas are still far from being fully worked out in practice.

* * *

The Second World War disrupted the educational services far more seriously than the first, and did vastly greater material damage to them. It also produced much more radical thinking. Administrators and teachers recovered quickly from the initial shock of war, and the sordid stories of evacuation gave them a wonderful opportunity, which they were swift to seize, to press the case for fundamental reform of the educational system. The moment was opportune. Public opinion, much better informed than in 1914–18, was everywhere ready to listen,

to agree, and to give support. Consequently, success became certain, and was quickly achieved: before the war ended the Education Act, 1944—the most revolutionary Education Act in English history—had set in motion a wave of reform which, in my opinion, has yet to reach its peak.

During the past fifteen years the country has been translating into practice the provisions of the 1944 Act. Far too slowly and incompletely, say many people. It must be acknowledged that two of its fundamental reforms—the raising of the school age to 16 and compulsory part-time education to 18—have not yet been begun; nor do they seem likely to be in the near future, despite the Crowther Report. But in the induction of educational reform speed is only one of several desiderata; and, important and desirable though it is, nevertheless essentially a supplementary one. Its function is to facilitate reform. No more is the magnitude or the multitude of the changes made a primary consideration, though both are also highly important. The factors of fundamental importance are the direction and the quality of the reform.

In respect of material provision I think the country could hardly have gone much faster. Those who take the contrary view must face three very hard facts: first, that until about 1950, that is, for at least one-third of the period under review, material progress was delayed and obstructed by all sorts of shortages and bedevilled by economic crises; secondly, that during these fifteen years the school population has increased by over 30 per cent, the university population has doubled, and the number of full-time students in further education establishments (to say nothing of part-time day release) has quadrupled. If the cry is still everywhere of shortage of accommodation, it must be remembered that those whose business it has been to provide this have throughout been faced with a constantly—and often unpredictably—growing task.

Thirdly—and this is a point which I fear educators tend to overlook—the country has had simultaneously to cope with many other gigantic programmes of national reconstruction: public health, housing, transport, industrial development, and scientific and technological research among them; not to mention the most demanding of all, defence, which, however much we may deplore the fact, still seems inescapable. These all affect not only money but also manpower and materials; and it is the latter two that have been the crucial factors.

I am not suggesting that more money, manpower and materials could not have been allocated to public education. I do suggest that, all things considered, education has received a reasonably good share of the available national resources. Moreover, it is feasible to doubt whether the educational services could have staffed adequately much more new accommodation than they have received. For the provision of up-to-date educational accommodation—school, college, or university—in itself demands additional staff. As it is, there are new handicraft and housecraft rooms standing idle for lack of teachers. And behind that question lies the much larger and more fundamental one: whether our stock of educational ideas would have enabled us, even with the requisite number of teachers, to cope satisfyingly with the opportunities made available.

Even regarded absolutely, the provision within a dozen years (for new building on a large scale began only in 1948) of over 5,000 primary and secondary schools—one in six of the total number—and comparable expansion of training college, further education, and university accommodation, is no mean achievement. Even more remarkable is the fact that teaching staffs have been increased *pari passu* with pupil and student populations: in figures this means that the number of teachers in primary and secondary schools has grown from 200,000 to 275,000, in universities from 6,500 to 9,000, and of full-time teachers in major establishments of further education from 4,500 to 15,000. And it should not be overlooked that in the schools a far higher proportion of teachers are trained teachers than has ever been the case before.

I think, too, that the country need not feel dissatisfied about either the size or the variety of the changes made since 1945. Secondary education, much of it very good, is now being given to all children instead of only to about a dozen in every hundred. Truly remarkable advance has been made in developing new forms of secondary education. University education has been thrown open to all who can make the grade, with over three-quarters of its students financially assisted, instead of about one-third before the war. Vocational education, is for the first time, being structured coherently, and should shortly be able both to cater

efficiently for all levels of ability and to provide a highway parallel to, but different from, that of the university to the highest academic and executive distinction.

Unfortunately, the two most important factors, direction and quality, forever elude exact measurement and remain the prey of personal opinion. But some large changes are so obvious as not to need the support of precise measurement. It cannot be gainsaid that since 1900 schooling in England has improved out of recognition; it has made immense strides in freedom and friendliness, in breadth and in depth, in richness and in precision. The same can be said of the whole vast field of further education. If one has to speak in somewhat more guarded terms of university education it is because at the beginning of the century it contained two wholly dissimilar elements, one mature, the other hardly more than embryonic. The two elements are still dissimilar, but infinitely less so, for the embryo has become at least adolescent. The modern universities have made as great an advance as any other branch of education.

That there has been huge progress in every field since 1900 is undeniable; the questions that trouble many people are whether since 1944 we have made progress as well as change; whether we are moving in the right direction and improving the quality of what we do. Only the future can answer these questions: but it is essential that we educators never forget two crucial points; that our present decisions will largely make that future, and that the larger part of the task set us by the 1944 Act still lies ahead. We have so far only roughed out a framework of reform; we have yet to fill in the details.

We are today, as always in the history of education, subject to severe, and conflicting, external pressures. We have only a very small share in the control and direction of the political, social and economic environment in which we have to do our work. But we have control of the instruments of education, fuller control than any other teachers in the world, and our decision to sharpen this one or lay that aside will have a powerful, and maybe decisive, influence upon the future of English education. Most of all, upon its quality, for which we alone are ultimately responsible.

The range, the depth, and the complexity of our task, in whatever educational field we may labour, have all increased immensely during the present century; and are still increasing. That makes decisions increasingly difficult. But they must be made, for today all mankind is on the march. If we do not lead, we shall have to follow.

TROPICAL CLIMATES AND BIOLOGY*

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Forty years ago, when I was young, our elders often told us that a zoologist's education was not complete until he had visited the tropics and worked on a tropical fauna. The richness and variety of animal life in the tropics is so great that they felt that a man who had not experienced tropical zoology could have no more than a very incomplete idea of the animal world and its distribution. It is my thesis in this address that work in the tropics is still of great value to zoologists, though not for exactly the reasons that led our predecessors to think so.

Today, zoologists are not interested so much in describing new forms and recording their morphology and distribution; most of us are more interested in the general biology of animals—in trying to understand the interactions between animals and their environments, physical and biological, how they manage to live in face of the often antagonistic conditions of their environments, what controls their distribution and evolution, and so on. If we do not go outside temperate climates such as our own, we tend to think that the conditions we find here are general, or at any rate normal, for animal life, and to neglect the fact that elsewhere in the world animals live in very different conditions. More than this, the range of conditions in a temperate climate is midway between the extremes of heat and cold to which life is exposed in other countries, and knowledge of the means by which animals survive in conditions nearer the extremes of the viable range often helps towards understanding their life in our own climate. In some ways study of arctic faunas shares these advantages with tropical biology, but in cold regions the fauna is so restricted, and investigation is so difficult, that I cannot believe that arctic biology can ever rival that of the tropics in value to the biologist.

I shall give some examples in which work in the tropics has given results that seem to me of interest to the general biologist, choosing examples in which the results are not such as might be expected from knowledge of the biology of temperate climates. I shall not deal with the economic importance of tropical biology, which is being discussed elsewhere at this meeting. Any economic use of a fauna or flora must be based on knowledge of the general biology; we need not fear, I think, that our results will be valueless even from the economic point of view.

Before I go on to my examples of tropical biology I must summarise the climatic conditions in which tropical and temperate environments differ, in so far as they seem to be biologically important, for it is necessary to realise the nature of these differences if we are to discuss their effects on the animals. I shall speak only of terrestrial and fresh-water environments, saying nothing of the sea where the differences are of kinds other than those I shall be describing. They would need a separate discussion.

Ultimately, most of the climatic differences between tropical and temperate regions derive from the greater altitude of the sun at midday in the tropics and from the world distribution of temperature and pressure, which is itself due partly to differences of solar heat at different latitudes and partly to rotational effects. How these ultimate causes produce their effects is the concern of meteorology and we need not go into it; we need only to know what the effects are.

On the equator the altitude of the sun at midday is never more than $23\frac{1}{2}^\circ$ from the vertical. It declines to this angle at the solstices in June and December, and is vertical at the equinoxes. In the

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equatorial region, therefore, any seasonal change there may be is double, as the sun passes north and south from the vertical. In temperature, however, the seasonal changes are very small, since, with the sun never far from the vertical, the amount of solar heat received does not vary by more than 8 per cent, and the variation in the length of the day is insignificant. At the tropics (23½° N. and S.) the seasonal changes are greater, for the sun at the winter solstice is at a height of only 43°, giving a variation of solar heat of about 27 per cent, and the length of day varies by about 2 hrs. In our country sunlight is about twice as powerful in June as in December, and the difference in heat received is greatly increased by the much longer daylight in summer.

As the result of these conditions, the seasonal change in mean temperature on the equator is not usually greater than 1–2° C. and is much less than the diurnal range which is often 10° C. In equatorial regions, however, temperatures are never very high. The annual mean is usually between 25 and 30° C. being prevented from rising higher by the humidity, cloudiness and other conditions. Extremes of heat are characteristic not of the equatorial regions but of the deserts in subtropical latitudes.

Though in fact the sun passes north and south of the vertical everywhere within the tropics, the double seasonal change is practically restricted to latitudes within 10° of the equator. Further from the equator than this, the sun does not pass far enough from the vertical at the summer solstice to produce a noticeable effect. But the range of seasonal temperature change increases as we pass away from the equator and may be as high as 8° C. at the tropics. It is however still less than in our latitude, where it may be as much as 15° C.

Rainfall is more important than temperature in determining the differences between tropical environments. Equatorial regions are in general characterised by fairly high rainfall, because they lie between the north and south trade winds where rising currents of air are cooled and their moisture precipitated—the region known at sea as the doldrums, also a region of relatively high rainfall. As the sun passes north and south, this area of high rainfall follows it, and near the equator the rainfall decreases. The result is that, although the seasonal change on the equator is small in temperature, in rainfall it is considerable. It is unusual in an equatorial climate for any month to be entirely without rain, but the difference between the driest and wettest months may be great. Baker and Harrisson[1] compared tropical climates in this respect and find that the rain of the wettest month is more than 2·5 times that of the driest in all but 3 per cent. The farther we go from the equator the difference increases, the dry season following the sun with a lag of 1–2 months. Total rainfall also becomes less, especially beyond 15° N. and S. where the subtropical dry belt is approached.

The character of the rain as well as its amount is very important in controlling environmental conditions in the tropics. All over the tropics cyclones with large variations of atmospheric pressure, such as we know here, do not normally occur. The rain is almost always convectional—due to upward movement of currents of air—but its frequency is very different from one region to another. In the rain-forests there is often rain on almost every day—there are on the average 249 rainy days a year at Pará near the mouth of the Amazon—and the rain usually falls as a storm of an hour or two's length, often with thunder. In some deserts the intervals between storms may be more than a year on the average.

It is the frequency of rain more than any other feature of the climate that controls the nature of environments in the tropics. Where rain is frequent, the environment is unable to dry between storms and the relative humidity is high. It is usually above 90 per cent in rain-forests where fog is frequent at dawn. Much of the water flows off the land into the large rivers that are characteristic of rain-forest regions. In doing so it leaches the land and the surface soil becomes denuded of salts. Forest waters are for this reason among the softest in the world; they may have a salt content only two or three times that of rain-water.

More accurately, it may be said that the nature of the environment is controlled by the proportion (l/g) of the gain of water to the environment in rain to its loss, not only in evaporation from open water surfaces but in transpiration from the vegetation and percolation into the soil. Where the rain is less than would be lost in these ways if the water were there, the environment will be a dry one and no water will flow off the land; where the

gain is less than the loss, the environment will be humid. In the rain-forests the value of l/g is of the order of 0·2, so that much water flows off the land and the environment is humid. In deserts l/g may be as high as 200, and in tropical grassland and savannahs it is probably often near unity.

Many other conditions in tropical environments are controlled by the value of l/g. Small diurnal and annual ranges of temperature are characteristic of humid environments, that is to say of those with a low l/g, not only in equatorial regions but generally in the tropics; in deserts the annual range may be as high as 40–50° C. and the diurnal range 25 or 30° C. Ultraviolet light is less in the more humid environments, cloudiness is greater and the hours of sunshine less (5–6 hours a day in rain-forests).

This account of tropical climates is very summary and incomplete; the few data I have given are almost wholly confined to the two extremes of climate, the rain-forests and the deserts. But I hope that it will serve to bring out some of the biologically important differences between tropical and temperate climates. The clearest of these are, besides the obvious difference in temperature, the much smaller seasonal differences in tropical and especially equatorial climates, and the greater part that water supply plays in controlling the environmental conditions. It is in fact true that in many tropical environments the effective rhythmical change of climate is not that of the seasons but that between rainstorms. I have myself found this to be clearly true in a country, the Paraguayan Chaco, where rain fell at intervals of about a fortnight. Pools and other small bodies of water would fill when the rain fell and dry before the next rain. Much of the smaller fauna of these pools—such, for instance, as the branchiopod Crustacea, e.g. *Estheria*—passed through their whole life-history in the few days that the pools were full, hatching at the time of rain and laying eggs before the pool dried.

Between the two extremes of climate that I have discussed there is, of course, a very wide range of intermediate tropical environments. These extend from woodland of many types to grassland and savannah, and to arid scrub where desert conditions are approached. In their general distribution this series of environments follows the reduction of rainfall as one goes north or south from the equator, but everywhere conditions are greatly modified by the local geography. Near the sea, and especially where trade winds blow on to the land, rain is more plentiful than further inland; the monsoon modifies climate in some countries; mountains may precipitate rain on their windward sides and produce deserts in their lee; and many still more local features of the geography, such as the nature of the subsoil and the amount of percolation it allows, or the efficiency of the surface drainage, will modify the environment in smaller areas.

As a first example of work on tropical biology that has given results not to be expected from our knowledge of the biology of temperate regions, I will take work on the conditions of life in shallow and stagnant fresh waters. Such environments are very widely distributed in the tropics. Mangrove swamps are found near the banks of many of the rivers, and papyrus swamps are widespread in Africa not only bordering the rivers and lakes but also filling shallow valleys far from the lakes. (In parts of Uganda 30 per cent of the land is said to be under papyrus.) In rain-forests large areas may be permanently flooded along the banks of the rivers, stretching many miles into the forest, and, besides all these, swamps of many kinds are found in open country.

Some of the features of these swamps are common to most of them. Almost always the water lies under thick growths of aerial vegetation—trees in the mangrove swamps and forests, papyrus which may grow to 12–15 ft. high, and in the swamps of open country grasses and other plants almost equally high. The water is often highly coloured—it may have the colour of weak tea—and is almost or quite stagnant even in the mangrove and papyrus swamps on the borders of rivers and lakes. In temperate countries undisturbed by man swamps may be equally widespread, but the conditions of life in their water are, as we shall see, very different from those in tropical swamps.

[My own interest in these environments has centred in the fact that they are of great interest for the study of evolution. It was almost certainly from swamps of this kind that vertebrates and probably many other terrestrial animals emerged from the water. But I shall not have time to discuss these matters in this paper.]

I take as a first example of tropical swamps

some in the Paraguayan Chaco in South America, in which Professor Beadle and I worked.[2] These lie in the almost flat grassy plains to the west of the Paraná river, which are in fact an extension

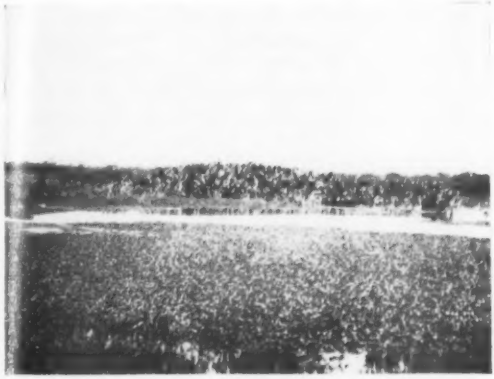

Fig. 1.—View of the Chaco swamps from the outer edge.

northwards of the pampas. Their latitude is near the southern tropic and the climate is therefore subtropical rather than typically tropical; it has a seasonal change of mean temperature of 8° C (27–19° C.). But in the hotter weather the characters of the environment are typical of those in true tropical swamps. The climate is moderately humid with an annual mean rainfall of 55 in. The rain falls at intervals of about a fortnight, more frequently in the hot season. Between the storms the temperature gradually rises until the next storm comes.

The swamps occupy depressions in the plains only a few feet below the general level. They drain very slowly towards the river so that their water is for all practical purposes stagnant though its level varies in dry and wet periods by 2 ft. or more. In the deepest parts of the swamp, which hardly ever dry, the water is at most 5 or 6 ft. deep. The substratum is a black mud full of marsh gas (methane), consolidating in its deeper layers and passing gradually downwards into a stiff and impervious clay.

The shallower parts of the swamp (Fig. 1) near its edges occasionally dry, and the water is covered by a floating blanket of aerial plants of many species, among which the swamp-lettuce (*Pistia*) and the swamp-hyacinth (*Eichhornia*) are dominant. Between these plants the blanket is completed by the smaller fronds of the water-ferns *Salvinia* and *Azolla* (Fig. 2). There may also be open pools where the blanket is missing. The more central parts of the swamp are filled with large clumps of a flowering plant (*Thalia*) reaching 10 ft. or so above the water and of the bulrush (*Typha*). Between these clumps the water is clear without vegetation and highly coloured (Fig. 3).

Investigation of the conditions in the waters of these swamps shows first that the content of nutrient salts is high. Phosphates, for instance, are present in concentrations of 2–4 mg. per litre, whereas in temperate waters concentrations around 0·1 mg. per litre are usual. Many other conditions such as the pH (6·2–6·8) and the bicarbonate content are suitable for the growth of phytoplankton, which we should therefore at first sight expect to be plentiful. In fact, in all parts of the swamp the water contains only a sparse plankton, both animal and plant, and in the central part there is almost none.

One probable explanation of this anomaly lies in the heavy shading of the water by the vegetation above its surface and the shallow penetration of

Fig. 2.—The floating blanket of the outer part of the Chaco swamps.

the light into the highly coloured water even if it is not shaded. In other similar tropical waters it has been found that the amount of light in the water may be below the compensation point for plants within a few inches of the surface. This is so in spite of the strength of the tropical sunlight.

Measurement of the dissolved oxygen content of the water shows an even more striking contrast with the conditions in similar waters in temperate countries. In the tropical swamps the oxygen is everywhere far from saturation even within an inch of the surface. It is in fact astonishing that in these and similar tropical waters one can often take a sample as close to the surface as is practicable—within at the most an inch—and find in it no measurable quantity of dissolved oxygen. In the central parts of the Chaco swamps even the surface water hardly ever in hot weather contained a measurable quantity of oxygen, and certainly less than 5 per cent saturation. In the outer region under the floating blanket the water was also almost always without measurable oxygen. Pools free of the floating blanket sometimes contained at midday 2–3 c.c. of oxygen per litre (about 50 per cent saturation) at the surface, but the lower water, even here, was often without oxygen continuously for many days in hot weather.

How is this lack of oxygen in the swamp waters brought about? I believe that it is the result of several conditions which are all present in these waters and not normally present in otherwise similar temperate waters. Oxygen can be introduced into a body of water by diffusion from the air, and produced in it by photosynthesis. It will be removed by the respiration of plants and animals and by the chemical and biological oxidations of decay. In the tropical swamps little oxygen is produced by photosynthesis owing to the weak lighting of the water, and decay, rapid at the high temperature, will actively remove any oxygen that gets into the water. Oxygen can reach the water only by diffusion from the air above it.

Entry of oxygen from the air must always take place, but in liquids diffusion, though rapid over a distance of a small fraction of a millimetre, is negligibly slow over greater distances. A thin oxygenated film at the surface will always be produced, but practically no oxygen can reach the lower layers of the water by unaided diffusion. It can only reach the lower layers if it is carried down by vortical disturbance, which may be due either to wind and current—and in the flowing water of rivers and streams all layers of the water are usually well oxygenated—or by convection due to the surface being sufficiently cooled at night to cause overturn of the layers of the water. These waters are stagnant, and the thick vegetation above them prevents any disturbance in them by wind. Thus, overturn is the only means by which oxygen could reach the lower water. But in these tropical waters, exposed to hot sunlight during the day, there is set up at midday a very steep gradient of temperature from the surface downwards (often 8–10° C. in a column of water 12 or 18 in. high), and in most nights no overturn occurs, so that the

Fig. 3.—The vegetation of the central part of a Chaco swamp from its outer edge.

water is permanently stratified and the lower layers receive no oxygen. That this is the correct explanation is confirmed by the observation that in open pools in the outer parts of the Chaco swamps some oxygen—up to 20 per cent saturation—was present in the lower water after unusually cool and rainy days or cold nights.

The deoxygenation of the water was paralleled by a high content of free carbon dioxide—up to 40 c.c per litre in the water of the outer parts and 70 c.c. per litre in the central parts. This was

clearly due to its inability to escape to the air by diffusion.

For animals breathing aquatically by gills these waters are therefore a very difficult environment,

Fig. 4.—Swamp vegetation in the Guiana forest. The ground is covered with 1–2 feet of stagnant water, but this is hardly evident in this photograph, since it is filled with decaying vegetation.

and this of itself is enough to explain the poverty of the zooplankton, even though, as was shown to be the case, the plankton is adapted to live at a low concentration of oxygen (5 per cent saturation). All the fauna shows adaptation to life in a deoxygenated habitat. These swamps are a well-known habitat of the air-breathing lung-fish *Lepidosiren*, and many of the other teleost fishes have evolved accessory air-breathing organs. Some of the smaller fishes, however, do not breathe air. They succeed in maintaining their aquatic respiration by living near the surface and using the thin oxygenated surface film, nibbling at it but not breaking the surface. The invertebrates also show many adaptations. A small oligochaete (*Aulophorus*) lives in the surface film of the outer region of the swamp. Being an oligochaete it needs a tube, and this it makes for itself from the spores of the water-ferns. It carries this tube about with it. Another oligochaete (*Drilocrius*) lives in very shallow water at the edge of the swamp making burrows in the mud. From time to time it extends from its burrow to the surface of the water where it captures a bubble of air in a modified part of its tail which is specialised for respiration. With this it retreats into its burrow. The large aquatic snail *Ampullaria* has a lung for air-breathing and lays its eggs in masses on the stalks of plants above the water. Some of the fish make nests which float at the surface of the water and have below them a foam of air-bubbles which the young use for their respiration. Others lay their eggs in the mud of the outer part of the swamp, but during the wet season when the lower water may contain some oxygen. *Lepidosiren* lays its eggs in an L-shaped burrow in the mud guarded by the male who is said to aerate the nest with air brought from the surface and excreted from the vascular filaments which he bears on his pelvic fins during the breeding season.

I have worked on similar stagnant waters in two other parts of the tropics—in the forests of British Guiana and in Uganda.[3,4] In the Guiana forests the swamps (Fig. 4) were shaded and protected from the wind by the trees above them. They were often as completely deoxygenated as the Chaco swamps but in them complete deoxygenation did not usually last for more than a few days at a time whereas in the Chaco swamps it might be unbroken for weeks. I believe that the reason for this difference is the greater frequency of rain in

Fig. 5.—Aerial photograph of papyrus swamps on the shore of Lake Victoria.

Guiana and the less heating of the surface by day. The African swamps (Figs. 5, 6, 7) were thickly covered with papyrus and the deoxygenation in them was again extreme. Everywhere, except in lakeside swamps near the open water of the lake, even the surface water contained no measurable

oxygen and the content of carbon dioxide was high. In these swamps the plankton was as sparse as in

Fig. 6.—A valley in Uganda filled with papyrus swamp, down the centre of which a stream flows.

the Chaco swamps and the larger fauna mostly air-breathing. The African lung-fish *Protopterus* and several air-breathing teleost fishes live in these swamps.

Thus, it appears that deoxygenation is a general condition in shallow and stagnant tropical waters, and this is borne out by the fact that air-breathing adaptations are found in the teleost fishes of similar habitats in many other tropical regions. In temperate countries deoxygenation does occur in shallow and stagnant waters occasionally during long periods of hot summer weather, but it is unusual. In the lake of the botanic gardens at Cambridge all the fish died some years ago in a hot spell; I believe that they were killed by stratification and consequent deoxygenation of the water, which is muddy and nearly stagnant. Normally in temperate climates, heating of the surface in the daytime is not strong enough to prevent overturn at night. But we have very few examples of work on such waters even in temperate countries, and still fewer on tropical waters. It seems to me that more accurate knowledge of the conditions in which overturn occurs in natural waters would be valuable. It might be expected that at high altitudes in the tropics, where the temperature is lower, conditions more like those in temperate waters would be found, and Beadle[5] has found that in some papyrus swamps on the shore of Lake Naivasha in Kenya at about 6000 ft. the water was 50 per cent saturated with oxygen. On the other hand, I have found in an open pool at Kigezi in Uganda at a similar altitude (5579 ft.) apparently permanent stratification and complete deoxygenation of the lower water. The reason for the difference is not apparent. Clearly more work is needed, and this should be both theoretical and in the field.

Equally unexpected results have been given by work on the sulphur content of tropical fresh waters in Africa. Beauchamp[6] pointed out that the sulphur content of many African fresh waters is very low; in several lakes it is not above 3 parts per million (mg per litre). He suggested that lack of sulphur is a limiting factor in the growth of the aquatic fauna. The subject was further investigated

Fig. 7.—Vegetation of a papyrus swamp seen from its edge.

by Hesse,[7] working on Lake Victoria. He found that the lake water contained 0·5–2 p.p.m. total sulphur and less than 0·5 p.p.m. sulphate. In

contrast the aquatic vegetation contained a normal sulphur content (average 0·1 per cent), so that sulphur can be a limiting factor only for the plankton-feeding and not for plant-eating fish.

When Hesse analysed the mud from the bottom of the lake, astonishing results were obtained. The sulphur content of the mud was extremely high at all depths down to 13 m. below its surface, but in all the samples far the greater part of the sulphur—in most samples more than 90 per cent—was in organic form. Inorganic sulphur is present but in relatively low concentration, sulphides often below the limit for estimation.

Clearly, the sulphur is not held in the mud by precipitation as sulphide, and this is confirmed by the fact that the water over the mud is often well oxygenated and the mud itself not in a highly reducing state. What apparently happens is that the sulphur is absorbed from the water of the lake by the planktonic fauna and flora and carried down to the mud in their dead bodies, so that the water becomes denuded of sulphur. In the mud the organic sulphur compounds in the bodies of the planktonic organisms are preserved without breakdown even for the several thousand years required for the deposition of 10–15 m. of mud. Plants growing in the water, if their roots penetrate the mud, are able to absorb sulphur from this store, and this is apparently the only means by which the sulphur can be carried back from the mud to the water.

Soils round the lake were also found to have a low sulphur content except where they were covered with forest, being leached by water draining towards the lake. The same shortage of sulphur is found in many soils in other parts of Uganda, and Simpson and Butters[8] have found experimentally that addition of lake mud to such soil improves the growth of plants growing on the soil. The organic sulphur compounds in the mud break down if the mud is dried, boiled or autoclaved. After being so treated it has been found to improve the fertility of fish-ponds.

I now turn from work on fresh waters to a quite different branch of tropical biology, investigation of the control of seasonal rhythms of reproduction and migration in tropical animals. This again raises problems different from those met in temperate regions. In many tropical environments, especially in regions at considerable distances from the equator, there may be, as we have seen, fairly large seasonal changes in the environment, and there is then no difficulty in showing that seasonal changes in the behaviour of the fauna are controlled by the environmental changes, as they usually are in temperate countries. In the Paraguayan Chaco, for instance, many of the amphibians and fishes breed after the first heavy rains of the summer season and it is easy to show that the stimulus for breeding is in at least some species cooling of the water by the rain. In the laboratory the easiest way to induce many of the frogs and fishes to lay eggs is to sprinkle the aquarium with cool water.

It is far less easy to see how biological rhythms are controlled in parts of the tropics where seasonal changes of the climate are slight. We have seen that in equatorial regions the only clear seasonal change is in rainfall, but it does not seem likely that variations in rainfall are the effective control of the rhythms, for there is rain in every month of the year, the humidity is always high and not significantly variable, and, though food may for some animals differ from month to month, it is always plentiful. Yet the fact is that most species have well-defined seasonal rhythms even in these environments, though a few breed all the year round and some others have double breeding seasons associated with the double seasonal change. Baker and his co-workers found,[9] for instance, that in the rain-forests at Noumea in the New Hebrides, a highly invariable climate though the latitude is 15° S., all the species he worked on were seasonal in their breeding, the birds and mammals at least as markedly seasonal as in temperate countries. A lizard (*Emoia* sp.) had a less clearly defined breeding season, though even it showed a seasonal rhythm of gonad growth. A bat (*Miniopterus* sp.), which spent the day in caves where the climate was even more invariable than in the forest outside the caves, was the most markedly seasonal of all, breeding on only a few days at the beginning of September. The breeding seasons were often not the same as those general in temperate regions, and in the case of a passerine bird *Pachycephala pectoralis* differed from its breeding times in places at the same latitude in Australia. He had evidence that the times of

breeding persisted at the same dates from year to year. He was not able to find any seasonal climatic change that could control the periodicity of the animals. Owing to the latitude the length of day at Noumea varies by $1\frac{3}{4}$ hrs., but he concluded that this was not the effective cause.

A large majority of species are seasonal in other invariable environments both in breeding and migration. Marshall and Williams found[10] that at Entebbe in Uganda on the equator the yellow wagtail (*Motacilla flava*), which 'winters' in Africa and spends the summer in Europe, was seasonal in the development of its gonad during its time in Uganda (December to April). They were unable to find any climatic change during this time to account for the periodicity. It is certainly not controlled by the length of day, for this does not vary significantly at Entebbe between December and April.

For some species details of their habits and biology provide the answer. Birds that nest on islands in rivers, or on river banks near the water, may be able to breed only in the drier months when their breeding sites are uncovered; and Marshall and Roberts,[11] find that the cormorants (*Phalacrocorax* spp.) on Lake Victoria at 0° 20′ N. are seasonal, breeding from May to December, and they come to the conclusion that the determining cause is the greater frequency of high winds between January and April, which destroy their flimsy nests.

Such reasons will not account for the general occurrence of periodicity in equatorial animals. If no explanation can be found in environmental changes, it may be suggested that the control lies in the animals themselves, in endogenous rhythms. But, if the breeding or migration is at the same date from year to year—and it seems to be so in most species—it is hardly possible, as Baker points out, for the control to be wholly by endogenous rhythm, for the period of the rhythm would have to agree very exactly with the annual cycle; any difference, however small, would mean that the time of breeding or migration altered from year to year. I know of only one instance in which the periodicity of a tropical animal is wholly due to an endogenous rhythm. This is the case of the wide-awake or sooty tern (*Sterna fuscata*), which nests on Ascension Island (8° S.). In this bird the interval between nesting times is not a year but 9–10 months.[12] No environmental stimulus could give this result.

Though the whole cause of the periodicity cannot be endogenous, this does not mean that endogenous rhythms play no part in its causation. It may be that in many species the rhythm is at base endogenous and is kept adjusted to the annual cycle by some external stimulus of which we are at present ignorant. Such a stimulus might be of almost any kind; it would probably differ from species to species and need not always be physical. Marshall and Williams, for instance, suggest that the northward migration of the yellow wagtails in Uganda is stimulated by the passage of birds of the same species from further south where they have been stimulated to migrate by environmental stimuli. The rhythm of gonad growth in Uganda would be endogenous and the birds would only respond to the stimulation when the gonads were in the appropriate condition. Another explanation of this example would seem to be that their migration southward is determined by environmental stimulation in Europe and the time of the northward migration by an endogenous rhythm of gonad growth starting from the time of their arrival in Africa.

In the many species which live all the year round in apparently invariable environments but yet are seasonal, it seems that there *must* be some environmental stimulus, physical or other, that controls their periodicity. For almost all of them we cannot say what the stimulus is and we can only admit our ignorance. Clearly this is a subject on which further work is needed.

Lastly, I will take an example from physiology, and from an environment very different from the swamps and equatorial regions we have so far considered. My example is the problem how small mammals are able to satisfy their needs for water in desert conditions. This has been studied by B. and K. Schmidt-Neilsen.[13]

It has always been difficult to understand how such animals as jerboas and desert rats can survive without drinking in deserts where the temperature may rise to 130° F. (54° C.) at midday. There are, however, some characters of the desert environment and the animals' biology that go part of the way to help us to understand their ability to do so.

First, the most striking characteristic of a desert

climate is the large diurnal range of temperature, very hot at midday but cool and even near the freezing point at dawn. Dew is frequent in many desert climates, and Buxton[14] showed that grass blowing about on the surface of a desert and apparently entirely dry contained water to 50 per cent of its weight at midday, presumably derived from the dew of the previous morning. Secondly, these animals are largely nocturnal; they avoid the extremes of midday heat in burrows.

They may obtain some water by eating the grass or from dew and may reduce their water loss by sheltering, but it seems unlikely that they can wholly maintain their water balance in these ways. The only other supply of water available to them is the metabolic water formed in the oxidation of their food; 1 g. fat yields 1·07 g. water in its oxidation, 1 g. carbohydrate 0·56 g. and 1 g. protein 0·40 g. Schmidt-Neilsen set out to determine whether this was a sufficient supply.

It should be noted that, as we should expect, desert animals are adapted in several ways to economy of water. They do not control their body temperature by sweating. Their sweat glands are reduced, and control of temperature by sweat is in fact impossible for small animals in desert conditions, for the amount of water loss required is far too large in proportion to their body weight. Schmidt-Neilsen finds that for a man of 70 kg. in the temperatures of a desert in daytime 1·47 per cent of his body weight must be evaporated per hour, for the kangaroo rat (*Dipodomys*, 0·1 kg.) 12·8 per cent, and for a mouse (0·02 kg.) 21·5 per cent.

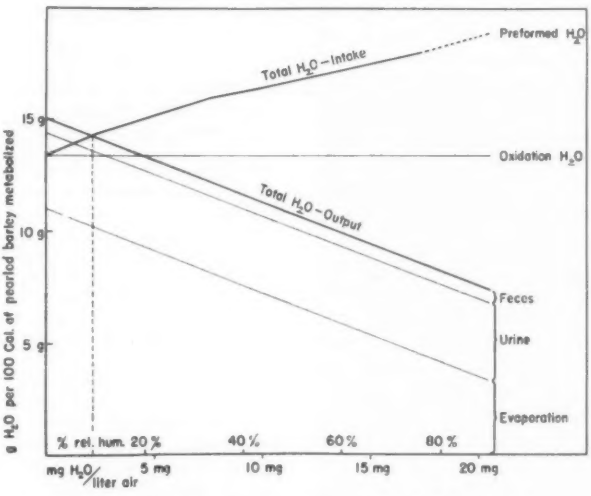

Fig. 8.—The water-balance of *Dipodomys*. (*After B. Schmidt-Neilsen.*)

Then, again, their urine is more concentrated than that of other mammals. Comparable results are given in the following table.

	Concentration in urine	
	Electrolytes N.	*Urea* M.
Man	0·37 (2·2%)	1·0 (6%)
Rat (*R. norvegicus*)	0·6 (3·5%)	2·5 (15%)
Dipodomys	1·2 (7%)	3·5 (23%)

Also, very little water is lost in the faeces. The faeces of *Dipodomys* have a water content only one-quarter of that of the rat's faeces.

Putting these facts together Schmidt-Neilsen draws the following diagram for the water balance of *Dipodomys* eating pearl barley without any supply of free water (Fig. 8). It shows that the water balance is positive at all relative humidities greater than 10 per cent. Remembering that the animals spend most of the daytime in burrows, we may conclude that they are sufficiently adapted

to live permanently in a desert climate without drinking. The rat is not quite in balance at a relative humidity of 100 per cent. It is of interest to note that a similar balance was worked out by Krogh[15] for the seal (*Phoca*) living in the sea, which is for a mammal a 'dry' environment since the osmotic pressure of sea-water is greater than that of mammalian tissues and water must leave the body by diffusion. Krogh found that the seal was also in balance, but in neither case is any allowance made for water loss by the female when she is giving milk.

I hope that these examples will have shown that tropical biology offers us many problems that repay investigation, and that the results are often not those that we should expect from our knowledge of temperate biology. There are many advantages besides these for the zoologist in tropical work, especially for the young zoologist. Perhaps, the most important is that tropical biology is at a much less advanced stage than that of temperate countries. It is much easier in the tropics to find promising lines of work, and less likely to find that the work one is doing is in competition with that of others, or has already been done—the field, in fact, is much less crowded. It is also true that one lives closer to nature in the tropics, and has greater opportunities to study animals in their natural lives. I know that it is for most of us impossible to get to the tropics for a visit of a year or longer—and a shorter stay is hardly likely to lead to worth-while results—but the fact is also true that when posts in tropical laboratories are advertised it is not by any means always easy to find people to fill them. I think that one reason for this is that the advantages of work in the tropics are not sufficiently realised.

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SIR WILFRID LE GROS CLARK, F.R.S.— PRESIDENT OF THE ASSOCIATION

The President of the British Association is an outstanding anatomist and student of the ancestry of Man. Born in Hertfordshire in 1895 he was educated at Blundell's School and St. Thomas's Hospital Medical School where he qualified in medicine in 1916, only to be caught up in the First World War, serving in the Royal Army Medical Corps. At the conclusion of the war he returned to the Anatomy Department of his hospital until, in 1920, he was appointed Principal Medical Officer of Sarawak. He served there for three very important years, for apart from the medical opportunities that were abundantly presented to him, his journeys acquainted him with the remarkable fauna of the region and first directed his attention to the primates as objects of anatomical, physiological and phylogenetic interest. He has never forgotten his interest in the people and the animals of Sarawak.

His return to England soon saw public recognition of his anatomical knowledge and of the width of his interest in associated studies. At first Reader in Anatomy and subsequently Professor at St. Bartholomew's Hospital Medical School, he transferred to the chair in Anatomy at St. Thomas's in 1929. He was invited to become Dr. Lee's Professor of Anatomy in the University of Oxford in 1934 and became a Professorial Fellow of Hertford College. He still holds these appointments.

With this academic background it is natural that in the course of the years many distinctions have come to him as a consequence of his outstanding contributions to anatomical and physiological knowledge. He has done detailed research into the structure of the brain, with special reference to the functions and relationships of the thalamus and the hypothalamus and also of the visual apparatus. It may be that his knowledge of primates, as well as of Man, led him to such investigations; certainly

Sir Wilfrid Le Gros Clark, F.R.S.

he has followed his work on the regeneration of the central nervous system with experimental studies on the growth and regeneration of the muscular system. The skull and skeleton of the lower primates have been especially studied and the results used comparatively to determine the lines along which the development of *Homo sapiens* could most likely be accomplished.

Sir Wilfrid has been frequently consulted by human palaeontologists, as is natural in view of his own wide knowledge of fossils. He has investigated

the remarkable Miocene primate discoveries of Africa, the Pleistocene Australopithecines, and was one of the trio that demolished the spurious Piltdown Man. More than this, his writings on Human Palaeontology have brought a proper proportion to many of the claims of those workers whose every anthropological discovery is unique and of phylogenetic significance.

As he has said, 'In the absence of an abundant fossil record, conclusions regarding phylogenetic development must always be provisional; and, as the evidence accrues with new discoveries, they will need constant revision.' The source of this observation, *The Fossil Evidence for Human Evolution*, published in 1955, contains much that urgently needed saying not only to anthropologists but to general vertebrate palaeontologists. 'In assessing the taxonomic position of a fossil specimen, account must be taken of the *total* morphological pattern', not merely that of the several units.

Sir Wilfrid's *History of the Primates* is one of the most successful of the British Museum (Natural History) handbooks. His *Fossil Evidence of Human Evolution*, published in Chicago in 1955, and his recent book, *The Antecedents of Man*, are all evidences of his wide interest and his sound judgement in fields of anatomy and zoology. His work has called forth many compliments, particularly in the form of medals. He has been awarded the Hunterian Medal of the Royal College of Surgeons; the Huxley Medal of the Royal Anthropological Institute and the Viking Medal for Physical Anthropology. He has been the Ferrier Lecturer of the Royal Society and the William Smith Lecturer of the Geological Society and has given special lectures in many universities at home, in Australia, in South Africa and in the United States. He has been awarded honorary degrees of Durham, Malaya and Melbourne, and is a member or honorary member of scientific societies in almost every continent.

Despite the calls upon his time he has managed to serve as editor of journals, twice as Hunterian Professor in the Royal College of Surgeons, and he was President of Section H (Anthropology) of the British Association in 1939. He is a Liveryman and Freeman of the City of London and has been Master of the Worshipful Company of Salters, one of the great City companies. Incidentally, this company finances Salters Institute of Industrial Chemistry, so that Sir Wilfrid has had administrative responsibility for sciences other than the biological. He was elected Fellow of the Royal Society in 1935 and was knighted in 1955.

Sir Wilfrid is interested in many things outside his work. The countryside and his garden have much appeal and he is fond of oil-painting so that he can record more permanently some of his outdoor impressions. Lady Le Gros Clark shares many of her husband's interests and they have two daughters.

BRITISH ASSOCIATION NEWS

Cardiff Meeting

The address by Sir George Thomson, President of the Association, and a selection of the Addresses given by Presidents of Sections at Cardiff are published in this issue of the journal. The remaining Presidential Addresses will appear in the November number of *The Advancement of Science*.

Belfast Junior British Association Meeting

The Northern Ireland Area Committee organised the second of this year's Junior British Association meetings in the Queen's University, Belfast, on Tuesday and Wednesday, June 28 and 29. Like its predecessors it was most successful and very much appreciated.

To meet special circumstances in Northern Ireland, where the major concentration of population is in and immediately around Belfast, but there are a number of schools at some distance from the city, a rather different pattern of programme was adopted. It was envisaged that a number of Belfast schools might send the same parties on each of the two days, while the more distant schools would only be able to send a party on one day. The opportunity was therefore taken to give a fuller treatment of some subjects than would have been possible in a single session. For example, Prof. C. L. Wilson gave on the Tuesday an introduction to analytical chemistry amd some account of the qualitative side of the subject, while on the Wednesday he dealt more with the quantitative side. Each lecture was complete in itself with its own demonstrations, but together the two did far more than could have been contemplated in one session. Similarly, under the general heading of 'Science and the Community', Dr. I. J. Faulkner spoke on Tuesday of the science of water supplies, soaps, detergents and clothes, and on Wednesday of food supplies and fertilisers.

Sir George Thomson, F.R.S. *(by courtesy of Eugene Kammerman and 'Saturday Evening Post'.)*

(See p. 191)

Another subject covered was archaeology, and here on the Tuesday Mr. G. F. Mitchell, reader in Irish Archaeology in Trinity College, Dublin, and President of Section C (Geology) for the Annual Meeting at Cardiff this year, spoke on 'The Archaeological Detective', while on the Wednesday Dr. Kathleen Kenyon spoke on 'Digging up Jericho'. By contrast, Dr. P. Walker, Royal Society Research Fellow in Cell Chemistry in the Department of Zoology at Edinburgh University, gave the same

talk on both days, speaking to the models he had brought with him.

Each participant on each day heard two lectures, and in addition there was a film show on the Tuesday, and a Brains Trust on the Wednesday, for the combined audience for that day.

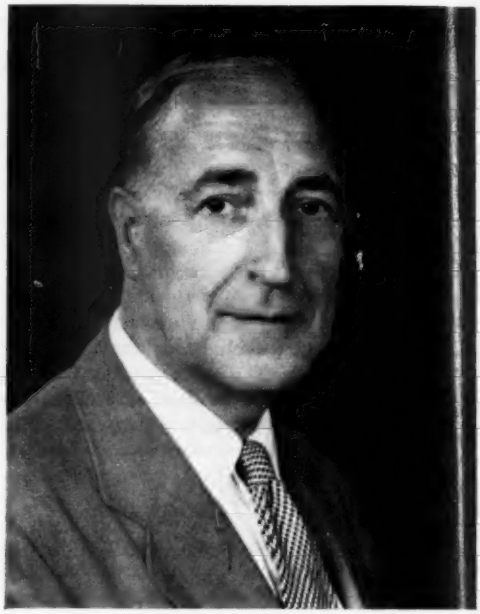
The opening session took place in the Whitla Hall of the Methodist College, with Prof. Estyn E. Evans, Chairman of the Area Committee, in the chair. Dr. M. Grant, Vice-Chancellor of the Queen's University, opened the meeting, and as a classical scholar commended the activity of the British Association in trying to bridge the gulf between science and the humanities.

* * *

Durham Area Committee Programme for Schools

In addition to a full programme for adult audiences, similar to that provided in 1958-9, the Durham Area Committee organised during the Christmas term of 1959 and the Easter term of 1960 a programme for schools in County Durham, Sunderland, West Hartlepool and Darlington

Prof. E. G. Bowen
(See p. 260)



Dr. James Taylor
(See p. 197)

which was quite remarkable for its size and scope. In all, ninety-five lectures for schools were provided in this time, to audiences totalling 7,200.

This programme was carried out by thirty-five lecturers all from the local panel of speakers organised by the Area Committee. Two speakers lectured eight times each, one seven times, one six times, three five times and four four times; but with the large number of schools visited a certain amount of repetition was both permissible and efficient. The most prolific speakers however scorned this economy of effort. The two who spoke eight times each were on atomic energy and astronomy; each spoke on four different aspects of his subject.

The most satisfactory aspect from the point of view of the British Association, apart from the fact that the programme was provided, was that it was entirely paid for by grants made by local education authorities.

The Council is most grateful to the Durham Area Committee for this most valuable work. Copies of the programme are being sent to all other Area

Committees; and if any Education Committee, Director of Education, or Chief Education Officer, Headmaster, Department of Education, Principal of a Technical College or Teacher's Training College, or other interested body or person would like to have a copy, it can be supplied on request.

* * *

Newcastle and Tees-side area Committees

The Newcastle and Tees-side Area Committees were, with Durham, the first to be formed, and the first to present a programme of lectures in 1958–1959. Their first full year of operation was 1959–60, and reports on their activities now available show what was achieved. Newcastle arranged 119 lectures, and Tees-side 145. Both programmes were almost entirely to adult audiences, and the audiences followed much the same pattern as in the previous year. Women's organisations again predominated, but there was a most welcome increase in the number of Rotary Clubs, and Church groups, which asked for speakers.

Both Committees produced brochures showing the speakers who were available, and the subjects on which they were prepared to speak—and inviting requests; and both committees ran into the problem inseparable from this method, that some speakers with a catchy title on a subject which does not sound too difficult are asked for too often, while other speakers are not chosen although their contribution to the work of the committee could be of at least equal importance. By contrast, the headquarters office lecture service does not issue a brochure, but on receiving a request for a speaker (if no specific subject is mentioned) suggests possible subjects, and then suggests a speaker available on the date given and suitable for the size and type of audience.

The trouble with this method is that it involves a great deal of correspondence for each lecture arranged. There is staff at headquarters to do this, but it is otherwise with the Secretary of an Area Committee, who is a busy man much concerned with other matters. One solution which some Committees are now adopting is to find a part-time Assistant Secretary, if possible a married woman with scientific or teaching experience and also secretarial experience, who is willing to undertake

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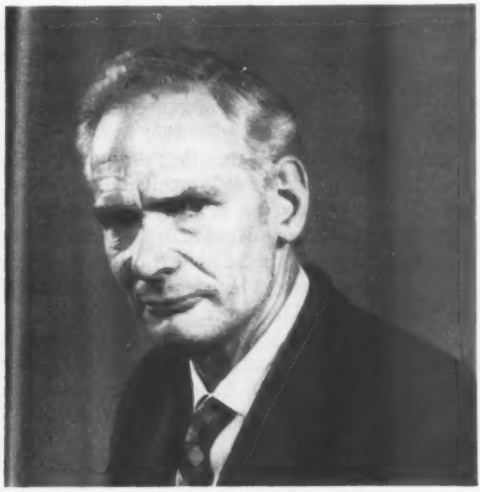

Prof. T. M. Harris
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the work for a short time on most days for a modest salary. Other ways are also being tried to improve the balance of programmes and to encourage a wider spread of scientific subjects and a greater interest in some of the less obvious and superficially less popular aspects of science covered by the panel of speakers.

The standard of lecture, both as regards content and presentation, has been high—and it seems clear that programmes of this magnitude and standard, presented in limited areas year after year, cannot fail to make an impact.

* * *

Worthing Science Society

This Society was established in 1958, on the initiative of the then Mayor of Worthing, Alderman H. D. Steele. The British Association was pleased to give what assistance it could, and to provide speakers for the meetings of the Society, whose members come from all the secondary schools in Worthing, including the Technical High School. Their first full year was 1959–60, during which lectures were given by Dr. D. C. Deuchar of Guy's Hospital on 'Recent advances in the investigation and treatment of heart disease' (their own requested subject), by Prof. C. E. H. Bawn, F.R.S. on 'The chemistry of molecule-building in Plastics and Man-made Fibres', by Dr. C. E. Ford on 'Genetics and the effect of radiation', by Dr. S. Gill (an old boy of Worthing High School) on 'General applications of computing machines', and by Mr. J. Williams of the R.A.E., Farnborough, on 'Aerodynamic aspects of vertical take-off aircraft and hovercraft'. There was also an evening of scientific films in December.

There are now 350 members of the Society which is now busy, in collaboration with the lecture service organisation at B.A. headquarters, planning a full programme for next autumn and winter.

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Dr. William Davies
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STATICS AND THE ENGINEER*

By Sir ALFRED PUGSLEY, O.B.E., F.R.S. *Department of Civil Engineering, University of Bristol*

1. Prologue

Textbooks on statics, even when intended for schoolboys, still seem to retain something of that air of formality and detachment associated with Victorian works on geometry. Forces are very disembodied and act either on infinitely small particles or on strictly rigid bodies. And this climate, I fear, still reflects, and is reflected in, the teaching of statics in schools.

There may be a certain cold grace about such a formalisation of statics, but it smacks of death rather than life. In the real world, all bodies are finite and deformable, and even breakable; and they are not just 'bodies' subject to forces of undefined nature, but are bridges, skyscrapers, ships and aeroplanes carrying real loads due to people and vehicles and goods, and having to withstand wind and wave and earthquake. In such an atmosphere, the practical one of the engineer, statics takes on a new interest and a new life.

It is in the belief that too seldom is statics given a chance to show not only its grace, but its practical liveliness and power, that I have chosen the subject of my address.

2. Introduction

The basic ideas of statics, which were first developed during the fifteenth and sixteenth centuries in such cultured cities as Venice and Rome, arose from discussions among Court Mathematicians and Physicians about the principles of operation behind the common building tools of their time—levers, inclined planes and wedges. The masons, engineers and architects of the day—and there was then little distinction between them—had been long accustomed to the use of these implements and so naturally gave little attention to the logic or scientific principles behind them. There was thus started a dichotomy between theory and practice, between statics and its application, which has been present with us, in varying degree and with varying results, ever since.

This was interestingly illustrated by the problem that arose when it was discovered, around 1742, that the Dome of St. Peter's was showing serious cracks. Pope Benedict XIV called in three mathematicians to examine and report on the matter. This they did, and came to the conclusion that the two iron tie rings placed around the base of the dome when it was erected had been overloaded and should be reinforced by the addition of three further rings. In support of their theory and proposed remedy, they submitted calculations using the then new principle of virtual displacements, but somewhat confused their argument by erroneous ideas about the elasticity of the rings. Apart from this error, typical of the notional difficulties then still to be overcome in statics and elasticity, their argument was generally sound and surprisingly modern in that it allowed for a factor of safety of 2 in assessing the total strength required of the new tie rings!

However, the novelty of this approach to dome construction met with resistance from practical men. 'If it is possible,' they said, 'to design and build St. Peter's dome without mathematics, and especially without the new fangled mechanics of our time, it will also be possible to restore it without the aid of mathematicians and mathematics—Michelangelo knew no mathematics and yet was able to build the dome.' In spite of this, within a year the architect Vanvitelli fitted five new tie rings and all was well.

This episode is something of a landmark in the history of applied statics: in a field of construction

* Address delivered to Section G (Engineering) on September 1, 1960.

dominated by tradition and practical skill, the strength and stability of a structure was subjected, for the first time on a large scale and in the public eye, to statical analysis. One wonders how it was that the Pope came to appeal to his three mathematicians; perhaps it arose in part from a fashion in high places at the time. Bell, in his *Development of Mathematics*, remarks on the leading mathematicians of the eighteenth century as being supported by the Kings and Emperors of the time with an eye to the need for mathematics in the advancement of civil and military engineering. 'These rulers', he says, 'were clear sighted enough to see that the simplest way of getting mathematics out of a mathematician is to pay his living expenses.'

It is of interest to examine, starting from this story of St. Peter's, the changing climate between mathematicians interested in statics and the like, and practical engineers engaged in construction. The eighteenth century, in this country, closed with the beginnings of the industrial revolution, when the engineer turned from his traditional materials to new ones like cast iron and wrought iron. As a result he was forced to think more about, first the strength and then the stiffness of his structures, and in the absence of any adequate theory (at least as known to him) he turned naturally to experiment, testing his beams and columns, and even his bridges, to assure himself of their adequacy. But such assurance was not enough; he wanted to understand in more detail just how his structures managed to carry their loads, if only so as to design better and more economical ones. And so he *himself* approached the mathematicians of his day, instead of, as in former centuries, having them imposed upon his activities from above. In suchwise came Gilbert, a President of the Royal Society, to analyse with Telford the strength of a suspension bridge cable, and Hodgkinson to study with Robert Stephenson the behaviour of tubular beams, preparatory to the design of the Menai bridges. Thus the first half of the last century saw classical statics extended into the engineering domain of strength of materials, and, by the end of the century, into the theory of structures. Hooke's Law of 1678 became the basis of a whole theory of elasticity, and early experiments on beams and trusses blossomed in such structures as the Forth Bridge.

But all this, it must be admitted, was at the top level. Brunel could do his own calculations for the Clifton Bridge, Benjamin Baker could analyse a whole range of possible designs for long span bridges, but the ordinary architect and engineer was for long in much weaker case mathematically and still did not really 'hold with' theory. Even Tredgold, who played a considerable part in the foundation in 1818 of the Institution of Civil Engineers and the furtherance of engineering science, could, on occasion, remark that 'the stability of a building is inversely proportional to the science of the builder'.

A hundred years later, in the 1920's, when I first worked in an engineering drawing office it was still natural to be advised by the engineer in charge to 'keep your degree dark, and at all costs avoid letting out that it was an honours degree'. But I have, it seems, lived to see a more enlightened age, when engineering graduates have overrun almost every branch of civil engineering, particularly those where statics, in its classical or more developed forms, can play a useful part. The old dichotomy has largely broken down: but this is not so much, I think, because mathematicians and engineers now speak the same language; in the sense that language expresses in part habits of thought they still, and rightly, do not. What has happened is that mathematicians—in modern terminology, applied mathematicians—are now seldom interested in statics, which seems dead to them. They have moved on to other fields—in this generation mostly to fluid mechanics—and have left statics to the mercy of engineers. As a result, in this century, statics has been blossoming in a new and rather strange, if sometimes muddled way, full of interest to the engineer, but ignored or viewed with some disdain by the mathematician. I propose in what follows to concentrate mainly on this new blossoming, using some modern structural problems by way of illustration.

3. Friction

But first let us look at an apparently more humble illustration. Nearly every schoolboy learns the classical 'laws of friction' in relation to motion, or lack of motion, on an inclined plane. And those of them who happen to test these laws by elementary experiment will have had the perhaps unexpected experience of finding that the so-called laws are by no means precise and sometimes

erroneous. Such experience is, of course, the very best of introductions to both the nature of science and the problems of engineering.

Putting aside the effects of lubrication as more appropriate to dynamics than statics, let us consider the problem of friction between dry surfaces. So long as scientists and engineers thought of the two bodies concerned as rigid bodies with smooth surfaces, no physical understanding of the basis of friction or improvement upon the old classical laws was possible. And each generation of schoolboys, after a few experiments, took the so-called laws—and perhaps by inference the whole of statics as presented to him by his master—with a pinch of salt.

It has taken twenty to thirty years' work in this century to break this impasse, to set aside here the notional difficulties of classical mechanics and start to think of friction as a problem in the strength and elasticity of materials. Once the touching surfaces were thought of not as rigid and smooth but as inevitably somewhat rough, like the surface of the earth or the moon in miniature, with protuberances which kept the bodies apart so that only a small proportion of the total surface actually touched; once it was realised that these touching asperities were bent, sheared, or otherwise deformed and even ploughed up by any effort at relative translation, a new light was shed on dry friction. At once the broad reasons for the tendency of most materials to behave somewhat in the manner of the old laws, and the reasons for their departures from these laws under certain conditions, began to become apparent. So far from the friction between two bodies being independent of their area of contact, the true contact area was seen to increase, due to local yielding at a steady stress intensity, with the force between the surfaces; and the frictional resistance, established mainly by the shearing resistance of the touching and interlocking 'peaks' on the surfaces, naturally tended to increase with the amount of true contact. The schoolboy who always wanted to argue that the friction must increase with the intensity of pressure between the surfaces was wrong in his terms but was nevertheless barking up the right tree.

The discovery of the real nature of friction between dry metal surfaces—due mainly to Bowden and his colleagues at Cambridge—of course derived not only from the change of viewpoint I have emphasised, but also from the resourceful use of modern techniques for the examination of metal surfaces and for the measurement of these areas of contact. But the change of outlook was fundamental to this successful re-exploration of the old laws of friction; and, as I hope to show in further examples, it is the overcoming of notional barriers that in statics, as in so many sciences, that has led to substantial advances.

4. Frameworks

Most sixth form schoolboys with a scientific bias, and all engineers, gain some familiarity with the way in which a frame, such as a roof truss or bridge girder of lattice type, is analysed for design purposes. The frame is treated as a structure with all its members in one plane and the joints where the members intersect are imagined to be smoothly pinned. The forces acting on the frame, whether due to gravity or wind, are then estimated and treated as concentrated at the joints. For the set of external forces so determined it is then easy, though somewhat lengthy, to calculate the internal forces—the pushes and pulls—in all the members of the frame. If the structure is to withstand the external forces, each joint must be in equilibrium; thus the forces acting at a joint—the external ones and the internal actions in the members intersecting there—can be calculated graphically or analytically by the application of the classical 'polygon of forces' rule of statics.

This approach to the design of a plane frame, which started in this country early last century, has since been developed into systematic graphical and analytical processes that are still widely used in design today. But there were within it two snags that have over the years caused heart searchings among engineers and drawn occasional contributions from mathematicians. Both relate to the arrangement of the members in the structure, though in different ways.

If we examine an ordinary railway bridge truss, we will find commonly that the members are arranged in a repeated pattern leaving open triangular spaces. Thus the top and bottom booms of the truss may be parallel and the members joining them be alternately vertical and inclined, to form what is called an N truss. This sort of construction, if truly pin-jointed, leads to what is

known as a just-stiff frame; that is, if you take any one member away, the frame loses its stiffness and becomes a mechanism with one degree of freedom and so will collapse under any external loading tending to excite such motion. On the other hand, if you add a member, as for example a supplementary diagonal member, the structure remains stiff, but what now happens is that one can no longer analyse it by the principles of classical statics alone. The condition that all the joints must be individually in equilibrium gives insufficient equations for the calculation of the internal loads in all the members. The structure is then said to have a 'redundant' member; and some practical structures have many such.

Fig. 1.—Front view of the structure of H.M. Airship *R 101* (launched, October 12, 1929; fatal last flight, October 4, 1930).

There is another form of redundancy common among frames: that due to external constraint. If we return to our railway bridge, we shall commonly find—at least in the older bridges—that one end of the truss is supported by a heavy pinned joint, while the other rests on rollers to allow, for example, freedom for thermal expansion longitudinally. This arrangement, however, has other effects. At the pin, if smooth, the support reaction can have two components, as vertical and horizontal; at the rollers, if frictionless, only one reaction (vertical) is possible. Thus when we come to consider the equilibrium of the truss as a whole, we have three unknown reactions to determine: for this, we have the three equations of equilibrium, corresponding to the balance of forces in the vertical and horizontal directions and of moments in the plane of the truss. But had we pinned both ends of the truss we should have had more unknowns than we could determine in this way by classical statics; we should have had a redundant reaction.

The calculation of such redundant actions, whether internal or at the supports, has been a problem and a battleground for engineers over the past 100 years. If we assume that we want to study the structure under elastic conditions, that is, loading conditions such that all the members remain elastic and return to their original configuration on removal of the loading, then the problem is one essentially of arguing that not only is every joint in equilibrium but that the forces in its members produce extensions and shortenings therein that are themselves consistent with the continuity of the frame as a whole and with its supports. Indeed, as was first pointed out by Clebsch in his famous book on elasticity in 1862, the whole problem can be transformed by regarding it not as one directed towards the solution of unknown forces, but as one seeking the component displacements of the joints. In these terms there are always as many equations available as there are unknowns.

This sweeping generalisation by Clebsch was, however, largely unknown in England, and English engineers for long dealt with redundant structures either by avoiding them in favour of statically determinate ones or by the direct matching of loads and deflections in the manner systematised by Maxwell (1864) and Mohr (1874).

By the time I was an undergraduate a new approach was becoming fashionable. Back in 1873 Castigliano had shown that if U is the strain energy stored in a frame due to a given system of forces, and P_1, P_2 . . . are the forces in the redundant members, then these unknown forces can be determined by the linear simultaneous equations

$$\frac{\partial U}{\partial P_1} = \frac{\partial U}{\partial P_2} = \frac{\partial U}{\partial P_3} = \ldots = 0,$$

and he enunciated this finding as a 'Principle of Least Work'. When this work was first translated by E. S. Andrews in 1919, it started at once a new fashion and a new problem. Energy methods had at last entered the realm of structures in the hands, not of the mathematicians, but of engineers. And the latter first tended to swallow Castigliano's idea of least work as a natural principle—that elastic structures always so managed their affairs as to resist a given system of loading with a minimum of work, i.e. as lazily as possible. Some were more cautious and sought other physical 'explanations'; of these the only one that was in itself completely sound and gained some popularity was that due to Southwell (1923), who pointed out that any redundant frame was liable to self straining during erection owing to the forcible insertion of bars or constraints of slightly incorrect geometry, and that Castigliano's theorem effectively expressed the physical condition that the total strain energy in a structure assumes a minimum value when there is no self strain.

A modern theoretical physicist accustomed to dealing with entities having no direct physical meaning, would be amused if he knew of these 'explanation' difficulties among engineers, particularly when he learnt the outcome of the further study of Castigliano's theorem. It has long been known that the differential equations resulting from Castigliano's energy approach were just the deflection 'matching' equations necessary to supplement the equilibrium equations that were otherwise inadequate in number. Only in the last decade has it been realised that the linearity of the frame problem masked the fact that Castigliano should for generality have dealt, not with the strain energy (U) of the problem, but with what Engesser called the 'complementary energy' (C) and that it was this, not U, that should be differentiated to give Castigliano's equations in generalised form, thus:

$$\frac{\partial C}{\partial P_1}=\frac{\partial C}{\partial P_2}=\frac{\partial C}{\partial P_3}=\ldots=0.$$

What would delight a physicist of today would be the knowledge that even civil engineers have thus to confess that they have been, and are still using an 'energy' that has no direct physical meaning, for C can only be expressed as $\int y.dP$, where y is a deflection under a load P.

I have said that over the last century there have been in the early theory of frames two snags to worry engineers; one is the question of redundancies on which I have already touched; the other is the question of how best to arrange the members from an economy standpoint. When designing a frame we have, in general, to transmit a load or system of loads from given points to given supports, and one would like to know how to do this with economy of material. We can see the immediate response of engineers to this problem in the variety of frame arrangements used for almost the same job: the many different forms of roof trusses and bridge trusses developed last century all point to practical efforts towards the solution of this 'best frame' problem. A guiding factor has been the idea that compression members, naturally liable to bow, should be kept as short as possible, and that tension members, free from this trouble and for which specially high strength materials (like wire) are available, should be the longer and more numerous. A succession of practical rules have been invented to aid economical design here; perhaps the best known last century was the rule that said economy in a girder was achieved by ensuring that, at the finish, the weight of its upper and lower booms was closely the same as the weight of its web members (the verticals and diagonals in an N truss). In this century, designers are well aware that a bridge girder excessively deep

$$\left(\frac{\text{Span}}{\text{Depth}}<5\right)$$ or excessively shallow $$\left(\frac{\text{Span}}{\text{Depth}}>15\right)$$

will be uneconomic. Only in recent years has a more scientific return to this problem appeared. In 1958, H. L. Cox noticed that a theorem of Michell, based on an earlier one by Clerk Maxwell, could be used constructively to provide, for a given load and support system, a 'best frame' arrangement. This at least is the theoretical position, though the actual process is difficult and has only been worked out in a few cases. The results look strange to the framework designer, at least until he remembers his predisposition to tension members rather than compression members. It seems, in the long run, as if the spider is the ideal frame designer; he uses the supports and the structure behind them (the branches of the bush) to provide his compression needs, and builds his web frame with 'fans' of tension members. But

the method so far developed on these lines lacks provision for other practical factors; in general, for example, we have not only a given set of loads and supports, but are restricted to a given space within which to construct our structure.

I have so far been discussing the statics of frames as though frames were restricted to plane frames; whereas space frames are sometimes used and present all the problems of frameworks par excellence. By a space frame I do not mean here the kind of frame in three dimensions that is constructed by an assembly of plane frames, as in the triangular framework of a radio mast or the rectangular frame of a tall building. I mean the sort of frame that is conceived as a space structure from the start and so unlikely to have more than a few of its members in any one plane. We have few examples of such structures in this country; some domes on the Continent are proper space frames, but the best examples occurred in airship structures in Germany, England and the U.S.A. in the 1920's and 1930's. In the course of this development the engineers concerned realised how little classical statics had done for them in this ambitious field, and strove, with the help of mathematicians and geometers, to advance the theory of space frames to cope with the new practical needs. Old theorems relating to the adequacy or redundancy of plane frames had to be replaced by new ones—even now less comprehensive than one would like—for space frames. Southwell, with his flare for intriguing titles, produced new processes such as 'The Device of the Separate Panel'—little known today—and his much more general 'Relaxation' methods in the course of this work. And Roxbee Cox charmed us with his excursion into the nature of just stiff space frames—still unsurpassed or substantially extended—'On the synthesis and analysis of simply-stiff frameworks' (1936).

Here we are at the frontiers of our knowledge of frames. No new wave of interest has arisen of sufficient intensity to push much further ahead. And at the back of the problem is the rarity of finding anyone among engineers or mathematicians who thinks naturally and constructively in three dimensions. Much was done in Germany at the end of the last century; England and America had an airship heyday; a new generation must take the next steps. Until that happens we seem doomed to see 'plane frame' structures all about us, when many of us feel sure that structurally and aesthetically better things could be achieved.

5. Suspension Bridges

Bridge designers are all looking forward to the completion of the first great suspension bridge in this country—that over the Forth—and hoping with South Wales and Bristol, to see the start of one over the Severn. To them, and I suspect to many schoolboys in the next generation, the large suspension bridge presents a challenge that, given the opportunity, few could resist. If statics can do any good in the world, here is something it can do with pride. The theory of the suspension chain and cable, whether viewed as a parabola or as a catenary, is something that has intrigued engineers and mathematicians for over 200 years. Most engineers, and many schoolboys, are able to discuss the mechanics of such a chain loaded only by its own weight, or by some uniformly distributed loading hung upon it. But one has only to try to determine the behaviour of such a cable under a single concentrated load to realise the real difficulties of the problem. Thus it has taken the sustained efforts of many able workers in this field to build up even an approximate theory of suspension bridges. And this has been paralleled over the last two centuries by the extension and improvement of the practice of construction.

Rankine, helped by the practice of Telford and Brunel and the model experiments of Barlow—all great names in English engineering history—produced (in 1858) the first scientific theory that made some allowance for the interaction of chain and stiffening girder appropriate to the spans (up to 200 ft.) of his day. But his theory, and those that followed upon it, treated such bridges essentially as elastic structures, with the cables liable to stretch and the girders liable to bend. It took the physical intuition of the designer of Brooklyn Bridge—Roebling—to realise that the weight of the bridge itself (in most bridges a nuisance) was a 'good thing' that could in the limit (with large spans) do all the stiffening that was required. The great George Washington Bridge, with its eight lines of traffic over a 3500-ft. span, but without any stiffening girders, has for long stood as a monument to the theory that was developed out of Roebling's hunch. Here was a piece of statics that bridge engineers had forgotten

about: you load a bridge at any one point, the structure 'gives' a little and the load moves downwards and so loses potential energy. In most structures, this energy is stored in the structure as 'strain energy'—energy in elastic form associated with stretchings, shortenings and bendings of members of the structure—that will cause the structure to spring back to its original geometry when the load is removed. But in suspension bridges (and in small unnoticed degree in some other structures) the relative inextensibility of the cables commonly forces the structure, when loaded and deflected downwards at one point, to rise at many other points. So the structure itself, instead of slightly losing potential energy by the downward movement, actually gains potential energy; and with heavy long span bridges this gain may be substantial. Thus if you pull downwards on a suspension bridge much of the resistance is not elastic in the usual way, but is really due to gravity. This was, until quite recently, only appreciated by the few designers of the latest large bridges.

We thus have, added to the problems of the irregularly loaded catenary, the wider problems of the action of a stiffening girder and the partition of energy between the elastic and gravity potential forms: a non-linear problem in statics still worthy of engineering scientists and still calling for further improvement.

6. Tubes and Shells

All engineers have at some time or another been interested, at least mildly, in some problem of elastic stability. Last century, with the coming of metal construction, few could refrain from having views on the column or strut problem: on how to predict, or provide against, instability in compression. In the first half of this century, in somewhat the same way and at least from aeronautical contacts during the wars, many engineers have become aware of the buckling problems of thin plates in compression or shear. These are all essentially, whether the material is elastic or not, problems of static stability. In contradistinction to the problems of strength and stiffness we have just been looking at in the realm of frameworks and suspension bridges—and without suggesting that these structures cannot themselves sometimes exhibit instabilities—let us turn, under our heading of tubes and shells, to some stability problems.

In the early days of higher technical education, the Central Technical College at Finsbury, with a staff including Silvanus P. Thomson and John Perry, was of high repute and became, indeed, the forerunner of Imperial College, London. The textbooks prepared by the staff at Finsbury were, for their time, of high standard and widely used. In one of the best known of these books, Perry's *Applied Mechanics* (1897), there is a short section dealing with the problem of the tubular strut, in which the advantages to be gained by 'inflating' such tubes are emphasised.

With this in mind, let us turn and look first at the problem of the thin tube (unpressurised) in compression. In the First World War such tubes became common in aeroplane structures, and this, together with advances in methods of manufacturing these tubes—drawing, extruding, rolling and welding—led to great interest in predicting and preventing premature local buckling of the walls of such tubes when in compression. There is clearly a great advantage with such struts in using, for a given length of column, as large a diameter as possible to prevent general bowing; but when this is done, for a given cross-sectional area of metal, the tube walls may become so thin as to precipitate local buckling and crumpling in the manner of an open-ended paper tube. In 1914, Southwell investigated the theory of local buckling of this kind, examining the stability of the walls by considering the effect of a generalised small elastic sinusoidal deformation of the walls. His result, as was shown by parallel experimental work by Andrew Robertson, was of the right general form, though in practice buckling occurred at roughly one-third of the theoretical critical value. The difficulty of making at that time thin enough tubes adequately to check this theory (a wall thickness preferably less than 1/100th of the diameter was required), together with previous experience of the effects of initial irregularities on solid strut behaviour, gave rise to the idea that this major numerical difference between theory and experiment arose from the inevitable departures in a real tube from true cylindrical form. And so the matter stood until fairly recently.

During the last war, however, when aeroplanes no longer used circular tubes so much as curved

metal plates in compression, the same sort of discrepancies between test and theory again arose repeatedly, and Von Kármán in the U.S.A., and Leggett here, opened up a new approach. They argued that, although initial irregularities might be partly responsible, the main cause might be found in the inadequacies of the linear theory used by Southwell for what might be essentially a non-linear problem. It had long been observed, for

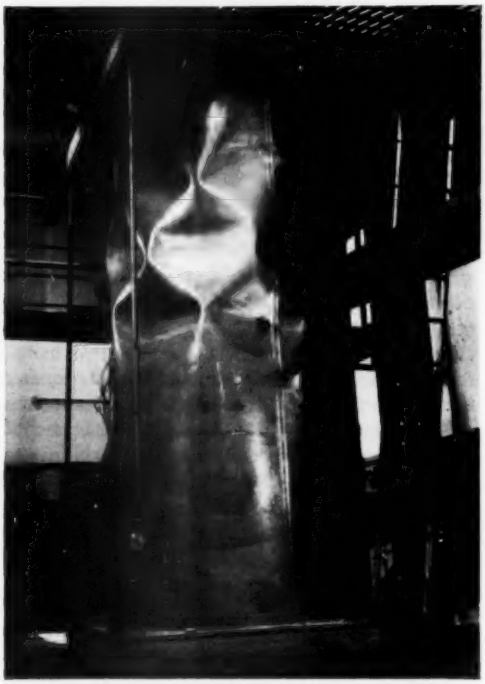

Fig. 2.—Buckling of large thin cylinder (unpressurised) in compression.

instance, that the walls of thin tubes, when buckling, moved inwards more than outwards, yet the linear theory gave no account of this, and indeed assumed equal amplitudes in both directions. With difficulty, but with considerable success, they showed that a non-linear theory, treating the amplitudes as appreciable compared with the wall thickness, not only accounted for the predilection to inward buckling but also at least partially accounted for the lower critical load found experimentally.

Since the last war, and particularly during the last few years, the art of constructing large guided and ballistic missiles has advanced this problem further. These missiles have as their principal structure, very thin tubes of large diameter; and these tubes, from the inertia of the missile warheads and the thrust of their jets, are primarily in compression. Moreover, under the military pressure upon industry in this field, ways have been found of making such tubes with walls not of 1/100th of the tube diameter, but of 1/500th or less. In these circumstances, the old problem of the static stability of a thin tube in compression came to a head in a vital way.

Now years ago, Perry, in the textbook to which I have referred, advocated the use of internal pressure in tubular columns, attributing the original idea to Prof. Maurice Fitzgerald of Belfast. Perry once argued, I believe, that the large columns that support the present Charing Cross railway bridge over the Thames would have been better filled with air than the concrete and brickwork actually used! Unbeknown to most missile designers, the same idea was revived by B. N. Wallis in a proposal for a single-span arch across the Thames (instead of the present reinforced concrete Waterloo Bridge). He proposed to make the main members of his arch of tubes filled with air under pressure, this being done partly to postpone buckling and partly to control the overall deflections of the bridge under load. Some tests on tubes so pressurised were at the time carried out under Professor Redshaw at Birmingham, but the internal pressure, beyond putting the closed tube into initial tension that had first to be overcome by the applied compression, appeared to do little to prevent local buckling.

Meanwhile, missile designers, ignorant of this experience, have gone ahead and pressurised their much thinner tubes; and by *ad hoc* tests have shown that pressurisation, in their case to the order of one-half an atmosphere, has not only provided initial tension but successfully postponed local buckling. Research into these practical results, using missile tubes in a systematic series of experiments, has in the last few years, in the U.S.A. and at Bristol, supported this experience and gone far to show the reasons.

When you pressurise a thin tubular column before loading it in compression, the first thing

that happens is that initial irregularities of form (not of wall thickness) are largely smoothed out; and this no doubt helps. But the other, and more potent, thing that happens, is that the fluid inside resists inward movement and so opposes any tendency for the walls to buckle inwards rather than outwards. As a result the mode of buckling, ordinarily a chequer-board or 'square diamond' pattern, changes until with high pressure the vertical diagonal of the diamond has become so shortened relative to the circumferential diagonal that the deformation becomes, not diamond-wise, but circumferential corrugation-wise, with the bulk of the deformation outwards (and so necessarily plastic rather than elastic). And the critical buckling load thus becomes substantially greater than for the unpressurised case. With such very thin tubes the engineer's 'hunch', rather than his theory, was right.

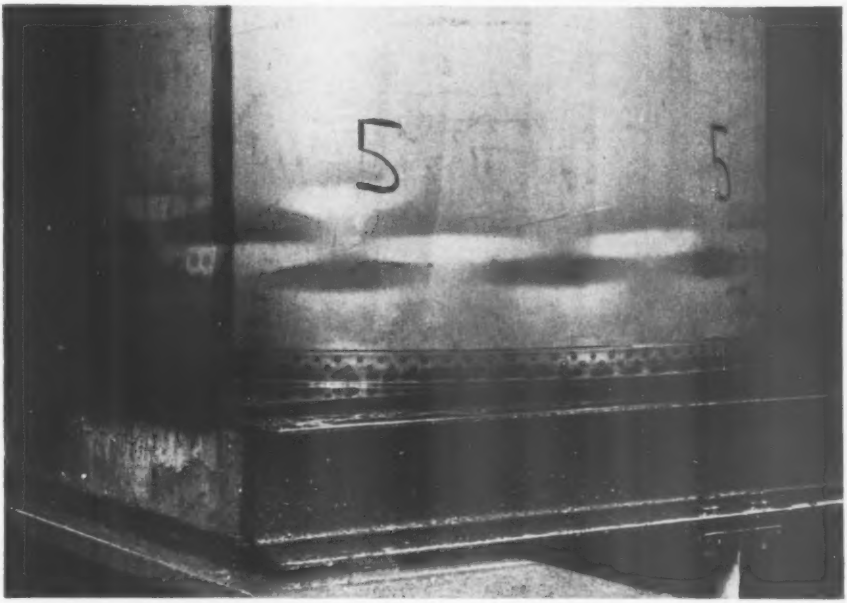

Fig. 3.—Buckling of large thin cylinder (highly pressurised) in compression.

In the field of tubes and shells, if we restrict attention to stability problems rather than strength ones, perhaps the most intriguing today is the problem of the deep-diving submarine shell. Here is a shell-like structure not under internal pressure and longitudinal compression, but primarily under heavy external hydrostatic pressure seeking to crush it and its contents. So long as submarines were thought of as surface vessels able, on occasion, to hide by diving below to a depth of 100 ft. or so, the problem of making the hull structure withstand the hydrostatic pressure was not very difficult. It was no more difficult than making the circular flue in a large boiler withstand the pressure of the water and steam round it—a stability problem that was studied by G. H. Bryan back in 1888. But now that submarines, nuclear powered, are thought of as living under water, and that at much greater depths, matters are more difficult. Should the structure be just like that of a conventional submarine, built as a tube with reinforcing rings at intervals? Or, in the extreme, should it be constructed as just one thick unreinforced shell in the manner of the cabin of Piccard's bathyscaphe? We are here concerned not only with elastic stability, whether linear or non-linear, but also with such heavy internal pressure that danger

may arise from the metal itself yielding and becoming non-linear in its response to compressive stress. In parts of such structures, however, plasticity might conceivably be an asset in some circumstances; Piccard, for example, after many trials and experiments, found that he could only make the windows of his spherical shell—built for

await more experiment and perhaps the new outlook of a future generation. Meanwhile, I hope and believe that engineers will still go on and make successful submarines and bathyscaphes, while science, in this case statics, chases in the rear, a very proper position, on occasion, as between engineering and its background science.

Fig. 4.—The Clifton Suspension Bridge as illuminated in 1959 to commemorate the centenary of the death of its originator, Isambard Kingdom Brunel.

submersion not to 100 ft. or 1000 ft., but to 20,000 ft. or more—safe by abandoning the idea that their material should be permanent and elastic in favour of making it plastic, leading to the occurrence of creep under load and hence to the need for occasional replacement. The statics of this deep submarine problem has been attempted in many ways, but is still not solved to the satisfaction either of mathematicians or engineers. We

7. Epilogue

It is common for engineering professors today to complain of the teaching of applied mathematics. Sixth-form schoolboys, when they arrive at a University, are often good at pure mathematics but poor or indifferent at its application. If this is true—and I think it is usually so—it may be due to the masters rather than the boys. How can a young teacher of mathematics, himself perhaps

inspired mainly by its beauty of form, have had enough experience in its application to have felt its power and constructiveness in practical life?

But it is not at school alone that applied mathematics is taught indifferently. In most Universities the engineering under-graduates are exposed to the teaching of mathematicians mainly concerned with mathematical and science students, and in very few are the engineering students required to take applied mathematics in their final degree examinations.

One learns best, one does best, when caught emotionally by the matter in hand, and this, I think, goes for pupil, teacher, and engineer alike. And in my field it is the great structure, the sweep of an aeroplane wing, the placid beauty of an ancient bridge, the sleek pride of an ocean-going liner, the ethereal grace of a cathedral, that first catches the heart-strings of a youth and all unbeknown binds him apprentice for a lifetime.

To one caught in this way, statics can have not only the cold, if dead, beauty seen by a mathematician; it can come alive in the hands of an engineer and help to give birth in the future, as it has in the past, to new forms, new structures of service to mankind. In the words of a famous suspension bridge designer of today—in one of his more poetic moments—from such efforts can come forms

> 'like a star-strung frame of song,
> a gleaming harp against the sky',

that will call from one generation of engineers to the next the world over, and so perpetuate my subject.

SOIL TILTH AND PLANT POPULATIONS

By J. C. HAWKINS *National Institute of Agricultural Engineering*

Farmers and research workers, seeking increased output from agricultural crops, have concentrated mainly on more productive varieties, the supply of plant food and water, the elimination of weeds and the control of pests and diseases. Recent advances in these fields have brought yields up to a level where farmers and market gardeners in particular, are casting round to find out what is now limiting output. Perhaps because they are practices which they have changed very little over the years, farmers are turning to their cultivations to find the answer. They see signs, too, that what they have been doing to soil with tractor implements lately may not always have been right. Weeds, especially grass weeds, have increased; it is becoming more difficult to make a good seed-bed on some soils; well-drained fields that used to be dry are now waterlogged and, with the modern low rates of seeding used for root crops, plant populations are lower than they should be and seedlings do not grow away well.

These symptoms that all is not well with the handling of British soils have made research workers think more about the problems of producing in soil the physical conditions that plants need for maximum germination and growth. The starting point has been to ask whether, with our present knowledge, these conditions can be defined at all precisely and in terms that the engineer can use in designing new machines or in measuring the performance of existing ones. Some farmers are sure that they know what is wanted in a tilth; but their ideas cannot always be supported by experimental evidence. Further, even if the ideal soil structure for germination and growth could be both defined and produced, there is doubt about the number of plants to establish per acre for the maximum economic yield. Some of the work that is being done to provide this information was described at the Annual Meeting of the Briti h Association for the Advancement of Science it York, and is outlined here.

The Measurement of Tilth

Many workers would like a simple way o measure the quality of a tilth: the engineer concerned with testing or research on cultivation implements especially needs some way of measuring the quality of a tilth in the field. He can then determine how effective an operation has been or measure the performance of any new type of implement for seed-bed preparation that has been developed. Attempts are being made all the time to find some way of measuring tilth or soil structure; but there seems to be very little hope that it can ever be defined adequately in terms of aggregate sizes. It is the properties that tilth confers on the soil that are important, rather than the size distribution and arrangement of the aggregates. It is possible, for example, that a given structure on one soil, as defined by a particular test, can provide all that plants need, when the same structure on another soil does not. The whole problem becomes much clearer if a good soil structure is defined as one which provides plant roots with the physical environment that they want. This in turn poses the question of whether we know that plant roots want. The answer is that the quantity of water needed by various crops in different climates is known. A soil's infiltration rate, permeability and water-holding capacity can be measured with fair precision, to see whether they are adequate for a given crop in a particular climate. The effect, too, of soil temperature, nutrient status and weed competition are known over a wide range of conditions. There remain, however, two properties of a tilth—the rate at which gases can diffuse through it and the resistance that it offers to

the passage of plant roots. Far less is known about the importance of these two in plant growth and, unfortunately, they are probably the two most affected by cultivation. There is not, therefore, likely to be any major advance in research on cultivations until more in known about them.

Farmers' Opinions

If precise knowledge of what constitutes a good tilth is incomplete, it could well be profitable to test opinions on cultivations firmly held by experienced and efficient farmers. The majority, for example, are convinced that an ideal seed-bed exists for a given crop and that departures from this ideal result in serious losses of yield or quality of produce or both. They may spend much energy, time and money in producing the conditions that they have in mind in the belief that the response of the crop will compensate for the extra cost involved. The experiments and critical surveys which have been carried out to check this, however, do not support this view, provided that the effect of extra cultivations in improving weed control are excluded.[1,2,3,4,5,6,7] Where crop responses to more intensive cultivation have been detected, they have usually been on poor soils having less favourable physical conditions.[8,9] Perhaps the most striking example of the difference between traditional belief and critical opinion is provided by the results of a survey carried out by Rothamsted Experimental Station in the 1930s.[6] A large sample of farmers on widely different soil types were asked to make a qualitative assessment of the seed-bed that they had prepared for sowing their barley. Later, at harvest, the crops were surveyed; yields were measured and samples of grain were examined for quality. The results showed that 'taken over the fields available, both the yield and quality of the malting barley were independent of the suitability of the seed-bed for malting barley as judged by the farmer himself'.

The importance of the seed-bed in the establishment of the barley crop has been examined further in recent work at Aberystwyth together with the effects of the seed-bed on winter oats and members of the beet family.[10,11,12,13] Five artificial tilths were prepared by dry sieving the soil and dividing it into five fractions: the smallest passed through a 1-mm. sieve, the remaining four through sizes between this and ½-inch with unsieved soil as the control. The emergence of barley plants was measured on these tilths. Fine tilths gave higher plant populations at emergence than coarse ones; but the differences were not large and, even on the coarsest tilths, enough plants came through to give a population capable of full yield. There were much greater differences, however, in speed of emergence in favour of fine seed-beds. From work with beet and tension table studies, it seems likely that this was due to mechanical impedance and less available water in the coarse seed-beds. The plant populations of winter oats at emergence followed much the same pattern over the six tilths, but survival was reduced by fine tilths and late planting and increased by consolidation of the seed-bed. The explanation of these results was clearly to be found in the damage caused to the plant roots by heaving of the soil by frost. This effect was significantly greater on the finest tilths.

This work, then, provides no support for the farmer's conception of what a good barley seed-bed should be although it does confirm the idea that a coarse seed-bed is desirable for winter-sown cereals. The reason, however, was shown to lie in the frost effects and not as most people think, in the provision of clods for shelter, the prevention of capping or in some other surface effect.

Cultivation

Although research and farmers' opinions have not yet defined what is needed in a good seed-bed, much can profitably be done to gain a better understanding of the action of the tillage implements now used and the effect that they have on the soil. The mould-board plough has remained the basic tillage tool in Britain because of its superior weed-killing powers over other implements.[14] It may cease to hold this place since research on selective weed-killing sprays suggests that weed control may shortly become a matter of using the right chemical. If this occurs, it will be possible to replace ploughing by a cheaper and quicker operation to break down soil into a seed-bed. In the meantime there is a need for the improvement of plough bodies to improve the quality of their work at the higher speeds now possible with modern pneumatic-tyred wheel tractors.

The breakdown of soil by implements is a process of hastening or finishing something which has been started by the weather. Frost, wetting

and drying, or a combination of the two, produce weaknesses in clods which can be detected by measuring their shear strength over a long period. For example, on land ploughed in the autumn a gradual decline in strength has been shown to occur throughout the winter, while the same land, ploughed in the spring, has had clods of almost equal strength to those in the autumn.[15] The natural weaknesses, formed by the weather, are exploited with most modern tractor implements in exactly the same manner as with horse-drawn ones, or with the spade, the hoe or the bent stick. With the possible exception of rotary cultivation, there has been throughout history, little or no change in the way that man has broken down the soil to plant his crops. Yet almost every year somebody produces a new tillage implement and claims that it has an entirely novel action.

Clods are broken by being loaded by cultivation implements against a resistance, provided in most cases by the surrounding soil. Thus, the effectiveness of a cultivation depends on the relative strengths of the individual clods and of the bulk of the soil. If a clod is strong and the bulk of the soil is too weak to support it, the implement will simply push the clod aside undamaged: when a clod is weak and the soil is strong enough, however, the clod will be broken. Some implements are more effective in breaking clods than others, because they load clods against the bulk of the soil in a direction in which the soil can offer greater resistance. Tines, for example, demand a resistance in the horizontal plane and are, therefore, less efficient at breaking clods than rolls or scrubbers which load the soil vertically—a direction in which it can usually offer the highest resistance. The result is that tined implements, especially harrows, tend to move and rearrange the soil particles and so have the effect of consolidation,[16] whereas rolls crush clods and are usually not heavy enough to produce much consolidation more than an inch or two below the surface. Disc implements impose high local loadings on clods, in a direction approaching the vertical, and so can cut through them. They would appear, therefore, to be less dependent for their effect on the presence of well-defined weaknesses in clods. This in fact is not the case because the range of particle sizes which make up an acceptable tilth cannot be produced economically by slicing up soil. Only when the high local loading of discs produces collapse of clods will they produce efficiently what is considered to be a good tilth.

Part of the soil-breaking action of rotary cultivators may be different from that of other tillage implements, although much of it is probably very similar. The narrow blades impose high local loads like discs and they load the soil approximately vertically like rolls. They are, therefore, effective in breaking clods for the same reasons as these implements, but the blades are travelling at about 11 m.p.h. instead of 3 to 4 m.p.h. The inertia of clods, struck at this speed, will enable them to offer more resistance to the blades, thus increasing their chances of being broken by them. In addition to striking some clods at 11 m.p.h., rotary cultivator blades throw others against the shield surrounding the rotor at speeds very little lower than this. The blow that they receive is probably equivalent to a drop on to a hard surface from a height of about 4 ft. and this can lead to further clod breakdown. Since in neither of these last two processes is the strength of the bulk in the soil involved, rotary cultivators may break more clods than other implements, particularly when the soil is dry and brittle. The resulting tilth will, however, be different from that given by other implements, because it is less dependent on natural weaknesses produced in clods by the weather. It will usually contain more very fine particles or dust[17] and may even approach something like the finest seedbed in an unconsolidated state used in the experiments at Aberystwyth.

Plant Populations

Assuming that research leads to the definition, measurement and more certain production of an ideal tilth for the establishment and growth of any given crop, it will be possible to specify more accurately the most profitable seed rate and hence the right plant population to use. Early work on this subject[18] showed that grain yields in wheat reached a maximum value at a given plant population, in other words the relationship between the two could be represented by a parabolic curve. This has since been confirmed by many other workers for many different crops.[19, 20, 21, 22, 23]

Vegetative growth, on the other hand, is not related to plant population in the same way. With increased populations the yield of dry matter

approaches a maximum without actually reaching it: the relationship between the two is, therefore, expressed by an asymptotic curve. This has been established for grass and clover,[24] for cotton and maize,[22, 25] sugar beet,[26] pine trees[27] and more recently at Leeds for wheat straw, rape, marrow-stem kale, thousand-head kale and potatoes. The work at Leeds suggests that the curve can be represented mathematically by the expression:

$$Ya = Yp \times X = AX \cdot \frac{1}{1 + AbX}$$

where:

Ya = Yield per unit area.
Yp = Yield per plant.
X = Plant population over unit area.
A = The apparent maximum yield attainable by a single plant in the particular environment in the absence of competition.
b = the linear regression coefficient for $1/Yp$ on X.

The probable reason why the curve is of this shape has been suggested by growth analysis studies. It seems likely that higher plant populations develop a complete leaf cover earlier in the growing season and so make more efficient use of the total light energy available for photo-synthesis; provided, as before, that other factors including soil structures are not limiting.

The Future

If weed control becomes a matter of applying the right chemical at the right time, it may lead to an entirely new conception of a good seed-bed. With more precise knowledge of the physical conditions that plant roots need, opinions, at present firmly held by farmers and market gardeners, may have to be changed. Then with a better understanding of the action of tillage tools on the soil, it may be possible to devise improved implements working on entirely new principles. It may pay, for example, to depart from the practice of breaking clods weakened by weathering against the resistance of the bulk of the soil. Instead, machines which reduce soil to the desired particle size in one operation, by pressure between two metal surfaces, may become desirable because they could make cultivations far less dependent on the weather and on the state of the soil. If that day arrives, farmers will be able to produce better and cheaper seed-beds, tailor-made for each crop, and can be more certain of how to establish the plant population that will give them the maximum return.

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WELSH EMIGRATION OVERSEAS*

By Prof. E. G. BOWEN *Dept. of Geography and Anthropology, University College, Aberystwyth*

A distinguished Past-President of this Section has said that for the geographer the distribution of Man is the most important distribution of all. It follows, therefore, that population studies must be of the greatest concern to us all, and no study of population can be complete without some analysis of migration, its causes, extent and meaning in population adjustments. The extent of human migration in modern times is frequently overlooked. It has been calculated, for example, that concealed beneath local national figures movements of migration throughout the world affected some five million people annually in the years preceding the First World War. The part that Wales played in such vast movements was, of course, infinitesimally small, yet there has grown up an extensive literature on the historical aspects of the subject without anyone, as yet, attempting a global appraisal of the matter from a geographical point of view. It seemed, therefore, particularly appropriate to attempt to fill this gap this year when the Association's Annual Meeting was being held in the capital of Wales.

At the outset the geographer is handicapped by the lack of statistical data on which to base his distributions, for, with the sole exception of the United States of America, no other country, when enumerating its foreign born population, provides a separate column for Wales. Consequently, I had to seek information elsewhere. It so happens that many Welsh folk overseas are desirous of keeping in touch with the homeland, and likewise, many Welshmen at home are desirous of maintaining close contacts with their fellow-countrymen overseas, so that a special organisation known as *Undeb y Cymry ar Wasgar* (The Union of Welsh People in Dispersion) has been formed with its headquarters at Dolgellau. This Society has very kindly furnished me with a list of all Welsh Societies overseas which are in touch with the Union, while, in addition, the Secretary was able to list for me the centres throughout the world at which the Union's Welsh periodical *Yr Enfys* (The Rainbow) is distributed. If all this material is mapped it can be taken to represent reasonably accurately the distribution of people of Welsh descent (whether immigrants themselves or the descendants of immigrants) living outside the British Isles at the present time (Fig. 1). There are marked concentrations in the North American continent; in eastern and southern Africa and in Australia and New Zealand. The geographer would naturally be interested in examining some of the broad considerations which appear to have determined the pattern. It should be remembered in this context that the majority of Welsh people (especially prior to 1914) were either farmers, miners, slate-quarrymen or metal workers, and those who emigrated overseas naturally sought areas where they could find similar employment.

Miners and Farmers

In the first instance, therefore, we find large numbers of people with Welsh associations farming on the Canadian prairies, or on the rich grain and pasture lands of the United States of America. The pastoral lands of New Zealand and Australia have been equally attractive, including, it would seem, the temperate mixed-farming lands of Tasmania. In the second place, the exodus of miners, quarry-workers and those engaged in the heavy metal industries easily accounts for the important concentrations of Welshmen in Pennsylvania and in the Utica region of New York State. The mineral belt of Central and South-eastern Africa has also

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attracted many from Wales. When the lead and zinc mines of western Central Wales became obsolete in face of foreign competition towards the close of the nineteenth century, many of the Cardiganshire and Montgomeryshire miners migrated to the gold fields of the Rand. It is not surprising, therefore, that the Rand has Welsh Societies at Johannesburg and Witbank and on the East Rand South Wales. Here we have Aberdare, Neath, Merthyr, Swansea and Cardiff all represented, with a Welsh Society located at New Lambton near one of the famous mines on the outskirts of Newcastle, and *Yr Enfys* is sent to Kurri Kurri, Paxton, Fairfield, Seven Hills and Merthyr for distribution to nearby families of Welsh descent on the Greta coalfield. It is not without significance in the

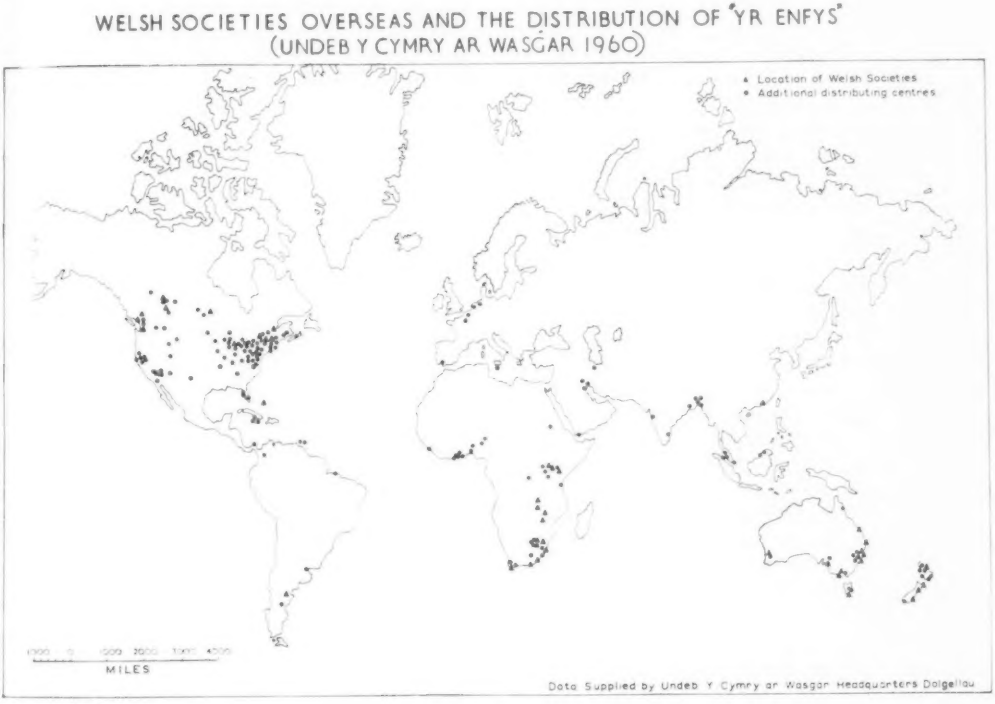
WELSH SOCIETIES OVERSEAS AND THE DISTRIBUTION OF 'YR ENFYS'
(UNDEB Y CYMRY AR WASGAR 1960)
Location of Welsh Societies
Additional distributing centres
MILES
Data Supplied by Undeb Y Cymry ar Wasgar Headquarters Dolgellau

Fig. 1.

respectively, and that *Yr Enfys* is distributed at three more places in the same area, Lyttleton, Germiston and Discovery.

In more recent times metal working has spread into the Rhodesias and beyond into the southern fringes of the Belgian Congo. Throughout this zone many Welshmen are found. If we turn to Australia, most geographers would agree that the major industrial area of the continent (the Newcastle–Sydney coalfield) has better claims than any other part of the State in which it is located to the title of New

present context that these important coals were discovered in 1886 by a very distinguished South-Wales-born geologist, Sir Edgeworth David, who was later Professor of Geology in the University of Sydney. To the south of the capital city lies the Illawarra coalfield forming the southern margin of the synclinal basin. Associated with this portion of the field is the very important twentieth century development of the Australian iron and steel industry at Port Kembla. Nearby at Woolongong is another Welsh Society. It is only to be expected

that the capital city itself should contain many families of Welsh origin. It is able to support two Welsh Societies, one of which is the Sydney Cymmrodorion. Likewise, in the city suburbs reside the retired folk and the city merchants and administrators. There are many Welshmen among their ranks and the list indicates that there are additional facilities for distributing the Welsh overseas magazine over and above that accomplished by the two Societies. Here then, in an unmistakably coal-mining, iron and steel working and business complex exist all the recorded Welsh folk resident in New South Wales. It is noteworthy that the Welsh people do not seem to be attracted by the semi-arid pastoral lands beyond the Great Dividing Range. Before leaving this section of our study mention must be made of the concentration of Welshmen in association with the mineral oil of Iraq and Iran. *Yr Enfys* is distributed at Tehran and Abadan in Iran, Basra in Iraq and at Kuwait on the Persian Gulf. Likewise, the map indicates that there are numerous Welshmen associated with Borneo and the Malay Peninsula and with the mining districts of Australia other than those in New South Wales. So clear is this aspect of Welsh emigration that we must not overlook the fact that there are several examples of Welsh people emigrating in large numbers to lay the foundations of important industrial regions in areas which do not emerge on Fig. 1. The most interesting of these is the emigration of Welsh iron and steel workers from the Merthyr and Dowlais area to Southern Russia in the late sixties of last century under the leadership of John Hughes. He was born in Merthyr Tydfil and served his apprenticeship in the iron industry at the famous Cyfarthfa works in that town. He gained further experience at Ebbw Vale and Newport and subsequently became manager of the engineering works at the Millwall Docks in London. He made a name for himself in the steel trade as the inventor of the famous Millwall steel. At this time Russia was attempting the development of her railway system, but had no steel works of her own and so had to import steel rails very largely from Dowlais where the 'Russian Trade' was well known at this time. In this context John Hughes was invited by the Tsar's Government to visit Russia and establish a steel works wherever he thought most appropriate. He decided to establish himself in the Donetz basin near to the iron ore of Krivoy Rog and the Black Sea port of Taganrog. In this basin were located extensive supplies of bituminous coal of Carboniferous age. The Donetz coalfield being one of a series of basins lying on the Hercynian front across Europe, and possessing coals of the same general character as those of South Wales, which Hughes knew so well. In 1869 he formed the New Russian Company and persuaded many iron workers and their families from Merthyr, Dowlais, Rhymney and Middlesbrough (where there were many Welshmen working in the iron industry, and where Hughes himself had been employed) to come over to Russia with him. A large settlement of these immigrant workers, who possessed a high degree of technical skill, grew up on the open steppe and was called Hughesovka (Yuzovka) after its founder. Here was the beginning of the industrialisation of the Donetz coalfield at the hands of Welsh emigrants. John Hughes died in 1889, but his work was carried on by his four sons. Enquiries made as late as 1896 yielded a list of people resident at Hughesovka with such characteristically Welsh names as David Davies, William Jones, David Waters, Thomas Thomas, Llewellyn Evans, John John, William Gibbon, Mary Jones, William Jones, Thomas Mort, David Jenkins, William Thomas, Francis Davies, Edward Edwards, Thomas Jones, William Williams, Edwin James Williams, John Richards, Richard Richards, Herbert Rees and Edmund Jones. The New Russian Company continued to exist and to develop the resources of the coalfield until 1917 when the Soviet Government took over the works and the Company came to an end.[1] In later years when the successive five year plans under Stalin achieved the industrialisation of vast areas of the Soviet Union, the pioneer Welsh settlement in the Donetz area changed its name from Hughesovka to Stalino—the name by which the present industrial city of 625,000 inhabitants is known. So complete have been the cultural changes of recent times that the headquarters of *Undeb y Cymry ar Wasgar* at Dolgellau has no knowledge of any Welsh society in Stalino, nor is there anyone in this great Soviet city known to be in receipt of a copy of *Yr Enfys*.

Religious and Commercial Emigrants

Two other classes of emigrant remain to be considered. Many have left Wales for religious reasons ever since serious emigration began, and their impress is clear in many regions. They contributed

to the foundations of Pennsylvania and Utah, while the presence of many Welsh folk today in Shillong and on the hills of Assam reflects the efforts of the Presbyterian Church of Wales, which has for many years maintained missionary centres and hospitals in the Khasi-Jaintia and Lushai hill-country. The remaining features of the general distribution shown on Fig. 1 are explained by the presence of many Welsh people in the business and commercial life of the large ports, particularly in Eastern and South-eastern Asia. Hong Kong, Singapore and Bombay are clear examples, while Welsh members of H.M. Forces stationed in places such as Aden must not be overlooked in attempting to interpret the distribution. Other temporary Welsh residents (who must be distinguished from true emigrants) help to fill in the picture. Teachers of all grades, as well as those involved in the Education Services, are a well-known export of modern Wales. To their ranks may be added the ever-increasing number of technical officers participating in the agricultural, land survey and hydrological services of the Crown domiciled in different parts of the Commonwealth. They may be regarded as forming an important element in the Welsh Societies found on the shores of Lake Victoria and along the West African coast.

It is obvious from the fore-going that there is, very naturally, a close correlation between the distribution of Welsh emigrants and the lands of the British Commonwealth. There are, of course, many minor, and at least two major, exceptions to this statement; the major exceptions being the United States of America and the Welsh colony in Patagonia in the Southern Argentine. I propose to devote the second part of this address to a study of these areas in some detail. The available data has been treated almost exclusively by historians and no-one has hitherto approached the matter from a geographical angle.

The United States of America

It is convenient to begin with the United States by mapping the only statistical data available on this subject. Fig. 2A shows the distribution of the Welsh-born population in each state as a percentage of the total foreign born population for the most recent data available—the Census of 1950. Owing to the immigration restrictions of the inter-war years and the virtual cessation of immigration from Wales after 1914, Fig. 2A appears but a pale reflection of the pattern of fifty years previously (Fig. 2B), yet, nevertheless, a reflection it is. From a study of both diagrams it is clear that there are three areas of outstanding importance in the distribution pattern of Welsh settlers in the United States. These are firstly, the Pennsylvanian concentration in the north-east; secondly, the Utah concentration in the west, and thirdly (but possibly less important) are the Welsh people diffused over the grain growing lands of the Middle West and the States through which the great transcontinental railway lines pass to the Pacific.

The importance of Pennsylvania is based on the fact that it became the home of some of the earliest Welsh settlers, and in virtue of its mineral resources and its metallurgical industries it continued to attract Welshmen from similar occupations at home well into the later phases of immigration. It would appear that Welshmen were slow in following in the wake of the earliest English settlers in New England and Virginia. But towards the close of the seventeenth century Welsh Quakers began to emigrate to what is now Pennsylvania where they bought a tract of land from William Penn himself. They were soon followed by many Baptists from South-west Wales. A hundred years later Welshmen with advanced political views looked to America as the home of political liberty in the same way as it had been the home of religious liberty a century before.[2] The close of the eighteenth century was also a period when settlers were pushing forward across the Appalachians to the rich lands that lay beyond. This brought many Welshmen seeking material gain, as well as religious freedom, and settling now not only in Pennsylvania but also in Ohio and the grasslands of Kentucky. The attractions of America to Welshmen in those days is well summed up by Benjamin Chidlaw's father who had lived in America from 1792–1799. He tells his young son that beyond the ocean there is a great and good country 'where there is no king, no tithes and where people can get farms and where apples abound'[3], in other words, political liberty, religious freedom, economic advancement and a temperate climate. Almost as soon as the mineral resources of Pennsylvania were discovered the Welsh iron master Richard Crawshay in 1800 evolved a scheme for settling Welsh iron workers there. This seemed to mark the beginning of a steady flow of labourers and technicians from Wales to Pennsylvania. Undoubtedly, the most significant

event in this connection was the migration of David Thomas from Ynyscedwyn in the Vale of Neath in 1839 with his specialised knowledge of smelting iron ore with anthracite coal. This knowledge was of crucial importance in the industrialisation of Pennsylvania, not only directly, but, through the influence of William R. Jones (another Welshman), the work of David Thomas became linked with that of Andrew Carnegie and the rise of the Pittsburg iron and steel industry.[4] This important manufacturing belt attracted at a later date many Welsh coal miners, steel workers and tinplate workers. In

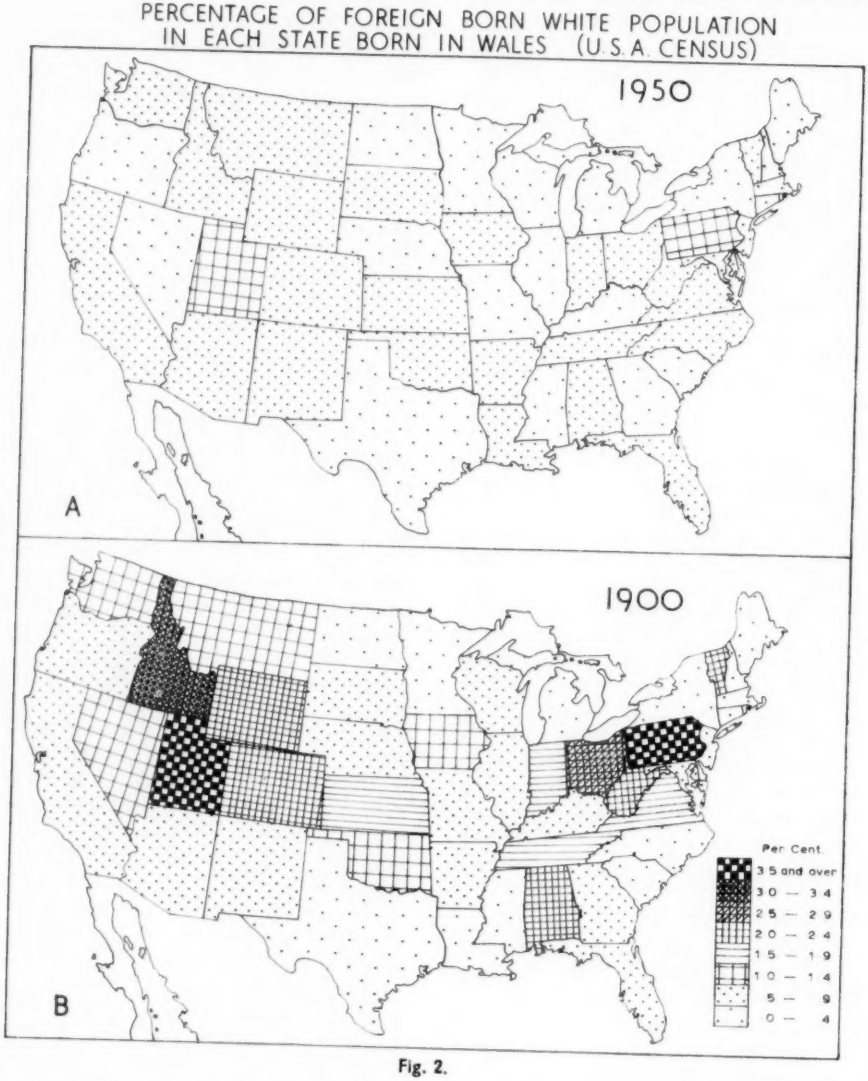

Fig. 2.

these new and widespread migrations it would seem as if economic motives had replaced the earlier religious and political enthusiasm, but this is not entirely true, for when we turn to consider the second most important feature shown on the distribution maps for 1900 and 1950 we have to consider the great Mormon exodus from Wales to the gathering of the Saints in Utah in the years between 1840 and 1870. Recent studies of the Mormon campaigns in Wales have shown how successful they were in certain parts of the country, particularly along the already industrialised northern outcrop of the South Wales coalfield and again in an entirely different environment in northern Carmarthenshire.[5] It is well known that the religious life of Wales in post-Reformation times was closely tinged with apocalyptic conceptions ranging from the visions of the Quakers to the cruder concepts of the more evangelical non-conformists. The Mormon missions to Wales, therefore, fell on fertile ground and their harvest was great. It was only the opposition of the other religious bodies, particularly the Calvinistic Methodists and the Baptists to these 'false prophets' that prevented their missions from being even more successful. Hundreds were persuaded to emigrate to the New Jerusalem that was arising in the western deserts. It should be remembered that these mass migrations were carefully organised and supervised so that many survived the rigours of the voyage and especially the long and difficult overland journey to the City of God. It is no wonder, therefore, that the United States Census returns for the years 1850, 1860 and 1870 show that the percentage born in Wales of the total foreign-born in the State of Utah was 6·1, 7·4 and 5·8 respectively; percentages higher than in any other State of the Union at these three enumerations. The percentage for 1860 is, incidentally, the highest that has ever been recorded for persons born in Wales in any State at any time since a census has been taken.

We can now return to Wales to examine a further source of emigration in the early nineteenth century. During the Hungry Forties those that dwelt on the upland farms experienced very hard times. They experienced a series of bad harvests following a series of wet summers, taxes were heavy and the landlords insisted on raising the rents, and much of the common land on which the poorer peasants pastured their animals was enclosed by Acts of Parliament. Many emigrated overseas and especially to the United States. A recent statistical analysis of the source of the emigrants to the United States from the various counties of Wales during the period 1795–1860 has shown that the contribution of the upland counties such as Merioneth, Cardigan and Caernarvon is abundantly clear.[6] If the statistics are studied chronologically the whole picture of the moving frontier of settlement in the United States becomes apparent. For example, no one from these areas settles in Ohio before 1817–1820, while after that date the number settling in this State rapidly increases; Welsh settlers reach Kentucky after 1841, and Wisconsin, Minnesota, Illinois, Kansas and Iowa between 1841 and 1850. The completion of the first trans-continental railroad in 1869 and the coming of the mechanical reaper merely accelerated the process, and Welsh settlers in company with many others of different nationality reached into Oregon and over the Rockies into the goldfields of California. It has been truly said that towards the close of the century there was scarcely a family in rural Wales which did not have a near relative in America.[7] As we have seen these Welsh relatives in the United States helped greatly not only in the establishment of the heavy industries, but also in permanently increasing the world's food supply on the virgin grasslands of the Middle West. It would be ungracious on our part, particularly at this meeting of the Association in Cardiff, to overlook the great contribution made by Welsh settlers in Canada and the United States to the solution of the world's food problem in the days before it reached such proportions as to demand the entire resources of this Association in an attempt to seek a solution. In spite of this great achievement and the obvious partiality of the Welsh people for the United States of America, the persistent dream of a real Welsh colony remained unrealised.

The Search for a Welsh Colony

Since the early days of emigration Welshmen had dreamt of a purely Welsh settlement overseas where all the characteristics of the people could be preserved, including their language and their institutions. Although many thought of establishing such a colony in North America, it was not to be. Most of the Welsh settlers found satisfaction in their new homes and, in turn, were proud to become citizens of the United States. The result has been that they

and their descendants have become absorbed in the American way of life while their ability to speak Welsh and to interest themselves in Welsh newspapers and Welsh institutions like the Eisteddfod, has become less and less.[8] While Welsh settlement in the United States was proceeding apace during the nineteenth century other Welshmen at home continued to dream of a fully Welsh settlement elsewhere in the world. Places as far removed from one another as Forest Creek in Australia and Palestine were thought of, but as time went on attention centred more and more on two areas in Latin America, well outside the influence of English colonisation and culture. The areas in question were Rio Grande do Sul in Southern Brazil and Patagonia in the Southern Argentine. A party of Welsh settlers actually went to Brazil in 1850 and attempted the formation of a colony in Rio Grande do Sul, but since the land proved unsuitable and the opportunities of maintaining Welsh culture unfavourable, the immigrants scattered and the project came to an end.

Patagonia

Attention was now directed to Patagonia where there were few Europeans and where the Spanish Government ruled only in name. A policy committee was established with Hugh Hughes (Cadfan Gwynedd) as secretary. It held its first meeting at Liverpool. There it was definitely decided to concentrate on Patagonia and to approach the representatives of the Argentine Government in this country on the matter. The secretary was also entrusted with the preparation of a Handbook on Patagonia indicating what was known of the land and the people.[9] The Handbook appeared in 1862 under the title of *Llawlyfr y Wladychfa Gymreig*, and in the same year two persons (Lewis Jones of Liverpool and Captain Love Jones-Parry of Madryn) were sent out to reconnoitre the land and to report on its suitability for settlement. Captain Jones-Parry's report was restrained and rather non-committal, but Lewis Jones was definitely of the opinion that the land was suitable for settlement. An official approach was now made to the Argentine Government and much propaganda was conducted through the medium of the press and by extensive lecturing campaigns throughout Wales. Altogether some five hundred people promised to emigrate to the new colony, but when the time for embarkation came only 153 appeared in Liverpool in April 1865. The expedition sailed in May, and after a long voyage reached Port Madryn on the shores of New Bay on July 28, 1865. The subsequent story of the colony is known almost in every detail and has often been told. It will not be repeated here, but rather an attempt will be made to assess the geographical knowledge available to the promoters of this scheme in the middle of the last century; and to show how, in their enthusiasm, much of it was deliberately misrepresented, and to stress how this, in turn, led to the appalling disasters which overtook the colonists in the early years of the settlement.

The use of existing geographical knowledge

The handbook provides a valuable source of information on the extent of the geographical knowledge available to the promoters, and it is surprising that its contents have never previously been analysed. After laying much emphasis on the fact that little reliance can be placed on the geography taught in school in those days (the handbook says that the schoolboy's picture of Patagonia was that of a wilderness, empty and forbidding with hordes of savages sweeping across it), the author proceeds to stress that if one is to obtain as clear a picture as possible reference must be made to first principles and to original sources based on travellers' accounts. To the author of this handbook first principles in geography were simple in the extreme. It is pointed out that southern Patagonia is in the same latitude as Mid-Wales, while central Patagonia is situated half way between the Equator and the Pole in latitude 45°, that is, in the same latitude as Bordeaux, Florence, or Venice, while the northern part of the country is in the same latitude as Japan, Messina, Palermo, Athens, Washington and Lisbon in the Northern Hemisphere. 'The winters, therefore, are mild and the summer heat not unbearable—in fact, many geographers would say that it possessed one of the healthiest climates on earth.' A most interesting feature of the whole presentation is the constant emphasis on simple latitudinal determinism. From the scientific point of view, the whole work is but an echo of the concepts of the ancient geographers, particularly the Greeks, who thought in terms of 'climata', which were basically a series of climatic zones corresponding directly to a latitudinal sequence from Equator to Pole. Based

on this concept the handbook proceeds to deduce the temperature and rainfall conditions in Patagonia from first principles. Here is a translation of the relevant passages.[10]

It may interest some people to have more detailed information about the temperature of the country, so we give herewith data prepared by Sir Woodbine Parish of the mean monthly temperatures (maximum and minimum) recorded at Buenos Aires for the whole year. The average temperature for the six summer months will be seen to be 77° F., for the six winter months 62° F. It should be remembered that Buenos Aires is nearer the Equator by 4° 13′ than the extreme northern part of Patagonia. The temperature for a whole year recorded at Port Famine at the southern end of Patagonia and given by Mr. Darwin (this is a reference to the great Charles Darwin and the data he collected during the voyage of the *Beagle* in the South Atlantic waters in the years 1831–36) indicates an average for the six summer months of 50° F. and for the six winter months of 33·08° F. It will be noticed that there is 19° 1′ difference in latitude between Port Famine and Buenos Aires, one half of this is 9° 35′. If one adds this to the latitude of Buenos Aires we get 44° 2′ S. which is exactly in Mid-Patagonia. Now, add the mean temperature of Buenos Aires for the six summer months to the mean temperatures of Port Famine for the same six months and divide by two, you get 63·98° F. Similarly, for the six winter months the figure for Mid-Patagonia is 50·97° F. Now, the average temperature for the six summer months in England and Wales is 57° F. and for the six winter months 38° F., so it would seem that the temperatures of central Patagonia are suitably adjusted to the constitution of the average Welshman.

It is a pity that in the absence of meteorological data for Port Madryn before 1903 the promoters of the settlement had no means of knowing, even after the landing, how near to the truth their *a priori* calculations turned out to be. Records since the beginning of this century show that the mean temperature for the six summer months at Port Madryn is 63·5° F. and for the six winter months 49·3° F. The 1862 estimates were, therefore, correct to within one degree, plus or minus. The handbook next deals with precipitation, where similar arguments are produced. A Mr. Kennedy had recorded the number of rainy days for each month of the year (for the years 1844–7 inclusive) on the banks of the Parana river. The average annual number of rainy days amounted to 45. Similarly, a Mr. Wilde had recorded exactly the same information for Buenos Aires for the same four years with an average annual total of 77½ days with rain. The compiler of the Patagonian handbook proceeds:

It will be seen that the number of rainy days at Buenos Aires is greater by 32½ than that recorded on the Parana river. The difference in latitude between the two places is 5° 37′. If one proceeds southwards 5° 37′ from Buenos Aires to latitude 40° 4′ (namely in northern Patagonia) the number of rainy days there will be 110. If one moves still further south by 5° 37′ to latitude 45° 41′ (in central Patagonia), the number of rainy days there will be 142, and if one moves yet 5° 37′ further south to latitude 51° 9′ (that is to the southern tip of Patagonia) the number of rainy days there will be 174. The accuracy of these figures is proved by what is said in Johnson's *Physical Atlas* on this matter. There it is proved that the number of rainy days increases as one moves away from the equator in mid-latitudes. The number of rainy days in Syria (Lat. 34°) is 54 and in the Straits of Gibraltar (Lat. 36° 7′) 61. As one moves northwards the number of rainy days increases so that in Southern France (Lat. 42°) the number is 76; in Central Italy (Lat. 44°) 89, in the plains of Lombardy (Lat. 46°) 96, in Hungary (Lat. 47°) 112, in Northern Germany (Lat. 49°) 131, in England (Lat. 52°) 158, and in North-eastern Ireland (Lat. 55°) 208.

Unfortunately, this type of reasoning proved far less accurate in estimating the number of rainy days to be expected in Patagonia than it did in estimating the seasonal temperatures! Modern records show that the average number of rainy days at Port Madryn is only 32, and not between 110 and 142 as the Welsh handbook had calculated. Even more interesting is the use made of travellers' accounts. One cannot but be impressed by the extent of the research undertaken by the promoters of the colony in this matter. Eighteen different authorities were consulted, beginning with Magellan's account in 1520; the extracts include references to the southern Argentine coast from Hawkins 1594, Captain Merlotte 1713–15, Lord Anson 1740–4, Sir Woodbine Parish, Lord Byron, Captain Willis, Sir Edward Belcher, Captain Bonn, Captain Read, Mr. Snow and Captains King and Fitzroy (with reference to the *Voyages of the* Adventure *and* Beagle *1826–36*). Tallis's *Atlas*, Bell's *System of Geography*, the *South American Pilot 1838–45*, the *Penny Encyclopedia* and the records of the Missionary Society to the Patagonians, 1855, had all been used, and extracts from these sources appear in the handbook. In addition, full use had been made of the work 'of that great naturalist, Mr. Darwin'. All this material had been discussed in the preparatory

committee meetings in Wales as well as printed in the handbook, and we know from the subsequent statements of Lewis Jones, the leader of the party, what in all this material influenced them most. This is what he said in a paper read to the Cymmrodorion Society in London in 1885:

Before this we had corresponded much about many areas, about Oregon, Vancouver Island, parts of Australia, New Zealand, Brazil and Uruguay, but when we came across the report of Captain Fitzroy (at the time when Darwin was with him) concerning the valley of the river Chupat in Patagonia, we came to the conclusion that at this spot a Welsh settlement could be established if the Argentine Government granted us permission and made it possible for us to own land there.[11]

Captain Fitzroy's Report

Captain Fitzroy's report was clearly the deciding factor. It is interesting to note what Captain Fitzroy actually said, and how his report was presented to the promoters of the settlement scheme in Wales. Here are the relevant portions[12]:

On the 24th Lieut. Wickham discovered the river Chupat and after waiting for the tide, anchored half a mile within the entrance. Next day he went a few miles up it in a boat, and found that although free from drift timber, it was shoal and narrow, few places being deeper than six feet at low water or wider than 100 yards . . . Mr. Wickham decided to move the *Liebre* as far up as he could and then make another excursion in his little two-oared skiff . . . the *Liebre* was moved 12 miles up in one day . . . Next morning Mr. Wickham went in his boat about 8 miles further, but in a direct line he was then not more than 12 miles from the entrance. Along the banks on each side, as he advanced, both he and those with him were much struck by the richness of the alluvial land, caused, doubtless, by the river overflowing its low banks; and by the quantities of drift-timber which actually looked like the stores in immense timber yards. Among the driftwood there were many large and sound trees left several hundred yards from the banks, therefore the periodical floods must be great. At Mr. Wickham's westernmost point the river and the country around had a beautiful appearance, as seen from the rising ground on the south side—an excellent position for a settlement. From this elevation the stream was traced to the westward running with a very serpentine course, through level meadow land, covered with rich herbage. Several herds of wild animals were seen, and their traces were observed everywhere in such numbers as to indicate a great abundance of animals.

The version of this put to the committee and to the prospective settlers read as follows in translation[13]:

On the 24th February 1833 Lieut. Wickham discovered the river Chupat in Lat. 43° 20′ S. He anchored within half a mile of its harbour and next day went upstream. We were amazed at the splendid scene around us. The land rich and fertile covered with vegetation and forests of tall stout trees. Near to the river these forests on all sides were thick and tall and could provide timber for any type of work. From a hillock nearby we saw the country around stretching green and magnificent before us, especially on the southern side where it would be possible to establish a very successful settlement. The river ran through this meadow land as far as the eye could see, with herds of animals grazing in the luscious pastures.

It will be clear that the original report has been considerably modified to give a distinctly favourable impression. In particular, there is no reference to the shallowness of the stream or to the clear evidence given of the dangers of severe periodical flooding; the driftwood lying on either side (looking like a timber yard) has been transformed into actual forests of tall stout trees, thereby obscuring the fundamental aridity of the land, and, finally, it should not be overlooked that Lieut. Wickham's report makes it clear that it was the rising ground on the south side of the river *some twenty miles from the mouth*, that, in his opinion, was 'an excellent position for a settlement'. When the hundred and fifty-three pioneers came to settle *at the mouth* of the Chupat they were to suffer severely from precisely these difficulties inherent in the geography of the land, difficulties that had been deliberately side-tracked, misrepresented or omitted by the leaders of the movement, in presenting to the committee and public in Wales the geographical facts at their disposal.

The early years of the settlement

No sooner had the ship carrying the emigrants anchored in New Bay than the truth became evident. It is recorded that many were shocked by the land's arid and forbidding appearance. A space near the shore was cleared and sown with corn. A period of strong winds but little rain followed and so the harvest failed completely, so did their water supply. A new site was clearly indicated and with the utmost difficulty the party moved to the mouth of the Chupat river. Heavy rain now began to fall and sheep and cattle were lost in the floods, and it was too late in the season to sow the corn that year. Several were in favour of transferring the colony

to the Falkland Islands, but, eventually, the Argentine Government came to their rescue. Further difficulties were experienced next year, which was too dry for the crops to grow, and it was only in the third year that things began to improve when one of the settlers had the idea of cutting a trench from the river to irrigate his land. Others followed his example and considerable improvements were effected. In 1869 there was a bumper wheat harvest, but on this occasion heavy rains caused the river to flood and the crops were washed away. The next year the river failed to rise, and little water reached the irrigation channels. The colonists countered this by making the entrances to their irrigation canals deeper. This, however, was not the complete solution, for in 1899 the flood waters rose so high that they burst the river's banks and caused the most serious damage. The Argentine Government came once more to the settlers' aid. Nevertheless, a similar catastrophe occurred in 1904.[14] The periodical flooding was clearly a most serious matter. We now have access to statistical and other data regarding the flooding in the Chupat area that was not available to Lieut. Wickham and Captain Fitzroy in the early decades of last century or to the Welsh settlers at the close of the century. The Meteorological Office's recently published statistics for Central and South America[15] give the mean annual rainfall at Santa Cruz (Lat. 50° 01′ S.) as 5·5 inches with no month possessing an average

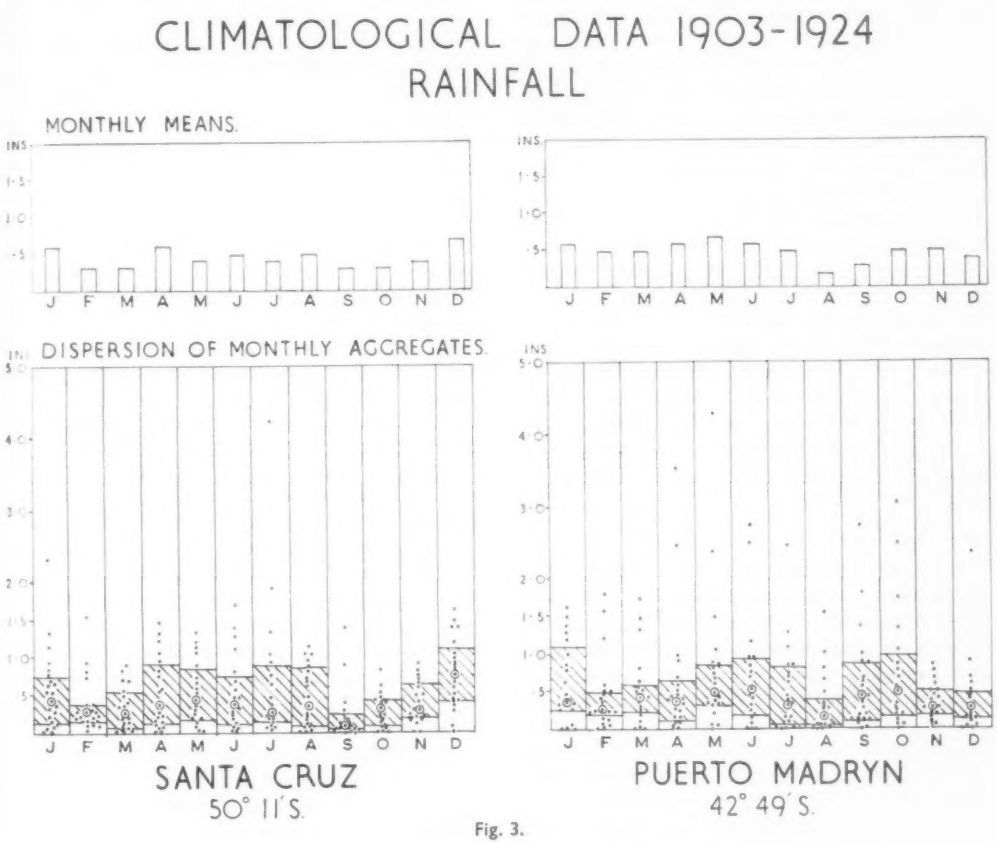

Fig. 3.

rainfall in excess of 0·7 inches. Port Madryn, some 7° further north, has very similar average conditions —an aggregate of 6·1 inches with no month with over 0·7 inches (Fig. 3). The variability of the rainfall in each case is, however, very different. At Santa Cruz during the period 1903–24 there were monthly means of over 1·5 inches on six occasions and over 2 inches on two occasions. These totals were exceeded at Port Madryn for the same years on twenty and on eleven occasions respectively. Thus, although both stations have very low monthly means due to the rain shadow effect of the Andes, occasionally large depressions break across and cause high rainfall, the unreliability of this occurrence would naturally be more pronounced the further north one proceeds. In addition, the occasional high aggregates at Port Madryn in summer may be due to rainfall associated with tropical air masses moving in from the east (Fig. 3). These occasional high aggregates are one of the chief factors (the melting snows in the Andes is another) which account for the periodic flooding of the Chupat. We can now appreciate what a serious omission was made in not informing the Welsh promoters of Lieut. Wickham's warning on this matter, even though he possessed only indirect evidence to support his statement.

Flood irrigation

A comment of another kind may be permitted on the fact that it took the settlers three years before one of them discovered the secret of flood irrigation under the circumstances. It is obvious, of course, that they could have had no direct experience of this type of farming in Wales, but a matter not usually stressed in this context is the fact that out of the original 153 settlers very few of them would appear to have possessed much farming experience of any kind, judging from the places from which they came. The list is most interesting in this respect. Mountain Ash contributed 39, Liverpool 20, Aberdare 17, Rhosllanerchrugog 10, Ffestiniog 10, Birkenhead 10, Bangor 7, Aberystwyth 5, Manchester 5, Ganllwyd (Dolgellau) 4, Llandrillo 4, Anglesey 3, Llanfairfechan 2, Llanfechan (Montgomeryshire) 2, with Bethesda, Brynaman, Bridgend, Denbigh, Holyhead, Seacombe, New York and Wigan one each.[16] The list could hardly have contained more than fifteen practising farmers at most. Indeed, the original settlers would appear to have been drawn from the ranks of those who had already left the land for the towns and growing industrial areas in North and South Wales respectively. They would appear to possess a somewhat different background from their compatriots emigrating from the hill farms of Merionethshire, Cardiganshire and Caernarvonshire to the United States in the preceding decades.

It took many years for the Chupat settlers to realise that they should remove their homes from the flood plain of the river to higher ground and, indeed, seek new and extensive grazing grounds on the high pampas further inland. Here they could keep their stock safe from the flood waters of the treacherous river. How wise was Mr. Wickham's casual remark in his report to Captain Fitzroy that it was the rising ground on the southern side of the valley some twenty miles up river that appeared to him to provide the best position for a settlement. In retrospect, however, it was the success of their irrigation methods that actually decided the fate of the settlement. It persuaded more Welshmen to emigrate from Wales and others from the U.S.A. By 1875 the population of the colony had doubled, trade increased, agricultural machinery and new crops (such as alfalfa) were introduced, and a railway line connecting the Chupat settlement of Trelew with Port Madryn on the shores of New Bay was opened in 1887, and two years later a weekly Welsh newspaper was established. Today the Welsh population numbers between seven and eight thousand, but they are outnumbered by Spanish and other residents. Recent reports indicate that the Welsh life and culture which the original colonists strove so hard to establish is slowly waning before alien influences.[17]

This address has attempted to examine the geography of Welsh emigration overseas in its broad global aspects. It would appear that this small nation has made a contribution to the peopling of new territories quite out of proportion to its size and population. Events have shown that the Welsh people have distinguished themselves as colonists in many different lands and would appear to show a facility for adapting themselves, often under very difficult circumstances, to new environmental conditions both physical and cultural. In studying the problems associated with such adaptation we are within the field of scientific investigation recognised by all to be the province of the human geographer.

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PASTORAL SYSTEMS IN RELATION TO WORLD FOOD SUPPLIES*

By Dr. WILLIAM DAVIES *Director, The Grassland Research Institute*

The subject as well as the title I have chosen for my presidential address have been of considerable personal interest to me during the past many years. I have had the privilege of travelling in many countries and in every continent during the past forty years, and in doing so have taken the opportunity to examine a variety of grassland situations ranging from systems of very low productivity to the extremes of high farming and maximum outputs. With these experiences as a background it would have been very difficult not to develop an interest in the multitude of seemingly different pastoral systems and particularly as these relate to overall food supplies. I therefore count myself extremely fortunate to be President of Section 'M' during this year when the Council of the Association have very wisely decided that the theme at this, the Cardiff Meeting, should be world food supplies.

From the dawn of history onwards, food and from whence it comes has been prominent in man's mind. It has provided the impetus for barter and trade, but also has been a major cause of war from tribal times to those of the complex civilisations of the present day. The whole course of history shows that fear of starvation triggers off some of the most banal of neurotic behaviours in the human mind. The classical wars of conquest had food supply as their origin, and this is basically true of all empire-building right up to the present time. The incredibly wide and indeed prolonged interest created by the utterances of Malthus, Crookes and others shows how important man regards his food supply. However, I must not pursue my pet philosophy but must proceed with the job I have been set.

* Address delivered to Section M (Agriculture) on September 1, 1960, at the Cardiff Meeting of the British Association.

Before looking at the pastoral problem in detail, I would first draw your attention to the land areas of the world as a whole. These I have rather empirically divided into (1) the tropics, and (2) the remainder. I say 'empirical' because I propose to define the tropics (in the pastoral sense) as the area lying between 30° N. and 30° S. of the Equator. This I believe to be a more realistic definition than is the solar tropic (23° 27′) because quite important changes in the grassland environment occur in the neighbourhood of latitude 30° whereas nothing comparable is apparent at the solar tropic. I make no claim to supreme accuracy when thus defining the tropics, and I would very much hope that someone will be sufficiently interested in my suggestion to cause work to be done which might lead to a more accurate definition than I am now able to offer. I feel confident that the change to the tropical from the extra-tropical condition occurs in the region of latitudes 30° N. and S. The precise line is likely to be a sinuous one and is bound to vary one side or other of the 30th parallel in conformity with local elevation, topography and the like.

TABLE 1

TOTAL LAND AREAS OF THE WORLD
(millions of hectares)

	'Tropics' to 30° N. and S. of Equator	*Remainder*	*Total (after F.A.O.[6])*
Area	6392	7111	13,503
Percentage	47·4	52·6	100

The land area of the world appears to be about equally divided between tropics (as here defined) and the remainder (Table 1). Only about one-fourth of it is used for arable and pastoral agriculture (Table 2). Unused land[6] occupies 43·2 per

TABLE 2

THE WORLD: LAND USE
(millions of hectares) (F.A.O.[6])

Land use group	*Arable**	*Permanent pasture†*	*Forested land‡*	*Unused but potentially productive§*	*Total*
Area	1352	2359	3949	5844	13,503
Percentage	10·0	17·5	29·3	43·2	100

* Includes tree crops such as olive, palm, and other fruit.
† Includes rough grazings, e.g. United Kingdom.
‡ Some may be used for periodic grazing, e.g. Finland.
§ Probably a considerable part used as periodic or even as permanent grazing grounds, e.g. Kenya, Rhodesia, Australia, Argentina, Brazil. The Falkland Islands (where the whole of the 1·2 million hectares are placed in this group [F.A.O.[6]]) carry 600,000 sheep, would be called rough grazings in United Kingdom and placed in permanent pasture. This indicates the sort of inconsistencies which are bound to arise in world-wide data.

cent of the total land area. I must point out, however, that I know of considerable areas in each of the continents (e.g. Australia, Africa and Falkland Islands) where farm animals have free access to and obtain a large part of their grazing upon lands which are clearly returned in national records as unused land and must, therefore, be so classified by F.A.O.[6]

At the present time I can do no more than note these sources of error; it is important to recognise them as such, but they do not materially affect the argument which I now hope to develop and in which I shall concern myself only with the areas returned in F.A.O. statistics as 'Permanent meadows and pastures'. This occupies nearly 6000 million acres and represents 17·5 per cent of world land. In my treatment of the global data, I have to make a number of (I hope) informed assumptions, one of which is that domestic grass-eating animals find the whole of their basic feed on permanent pastures. It could be argued that corrections might be made for leys on the one hand (included as arable in Table 2) or for the fact that animals have access to much of the 'unused land'. I have not attempted such corrections because of the many subjective decisions that such a course would demand.

I recognise that my treatment leaves room for criticism if only because of the many generalisations I am bound to make and the consequent over-simplifications in regard to many an important issue. However, the fact is that I have decided to deal with the pastoral systems of the world on a broad ecological basis involving grassland type, on the one side, and on the other the estimated number of animal units maintained on that grassland. Having approached the problem in this manner I find that global pastoral systems can be placed in five broad groups as follows:

1. *Very intensive*: grassland/animal systems within which the grasslands maintain about 80 'cattle units' per 100 acres. Examples can be found in many countries (e.g. Holland, New Zealand and the United Kingdom) but usually only on the best grassland farms.

2. *Intensive*: where the grassland maintains about 40 'cattle units' per 100 acres. This group is represented by average conditions in the United Kingdom and other 'advanced' grassland countries throughout the world.

3. *Moderately intensive*: grassland systems carrying about 20 'cattle units' per 100 acres of grassland; represented by quite large areas throughout coastal Australia, North America and in Europe.

4. *Extensive:* relatively low output grassland with a carrying capacity of approximately 10 'cattle units' per 100 acres. About half the total area involved lies within the tropics. This group is represented by the cattle ranches of the Western hemisphere, in Africa and in Australia. It is also represented by the elevated moorlands of Europe (including the United Kingdom hill grazings) and of New Zealand.

5. *Very extensive*: the assumed carrying capacity is 5 'cattle units' per 100 acres, but it may often be as low as one per 100 acres. The herbaceous cover is frequently very sparse and the grazing animal relies to a considerable extent on feed provided by a wide variety of shrubs and trees (e.g. *Acacia spp.*). The pastoral lands of Patagonia, Northern Australia and many parts of Africa belong to this group.

TABLE 3

GRASSLANDS OF THE WORLD: AREA OF PERMANENT PASTURES TOGETHER WITH ROUGH GRAZINGS OCCUPIED BY PASTORAL GROUPS 1–5

(millions of hectares)

Pastoral group	1 *Very intensive*	2 *Intensive*	3 *Moderately intensive*	4 *Extensive*	5 *Very extensive*	*Total permanent pastures**
Area	38	284	438	830	769	2359
Percentage	1·6	12·0	18·6	35·2	32·6	100

* After F.A.O.[6]

Table 3 shows the area and percentages of grassland occupied by each of these five pasture groups. It is of interest to note that more than two-thirds of the whole is occupied by the two 'extensive' groups and that only 1·6 per cent is classed as 'very intensive'. The efforts of the agricultural advisory services in all the 'advanced' grassland countries are to a substantial degree designed to upgrade from Group 2 (284 million hectares) to Group 1 (38 million hectares). Obviously the advisory officer and his farmer client have a long way yet to go!

In Table 4 I have made an estimate of the annual value of the major items produced in world agriculture, using the broad divisions (*a*) *animal products*, including meat, milk and milk products, wool, hides, skins, tallow; (*b*) *grains*, including wheat, rice, maize, millet etc., and (*c*) *plantation crops*, including sugar, fruit, pulse, oilseeds, cotton, coffee, sisal, tea, rubber, etc. The total sterling values (at London wholesale prices 1960) of these products is shown as in excess of £50,000 millions, of which roughly 30 per cent is accounted for by the grains, nearly 20 per cent by the plantation crops and nearly 40 per cent by animal products (meat, milk, etc.). These figures are impressive from a number of points of view including (A) in every country the standard of farming applied to arable and/or plantation crops is much higher than that applied to grassland. The United Kingdom provides an excellent example, for the standard of arable farming on the majority of the United Kingdom farms is at least twice as good as the grassland farming when assessed by present production against the known potential. (B) Much more research and technological effort in the past has been applied to tillage crops than to grassland.

TABLE 4

ANNUAL VALUES SELECTED PRODUCTS*

(*at approximate London wholesale price 1960; data F.A.O.[6] adjusted for U.S.S.R. where appropriate*)

(£ million sterling)

	£ *millions*	£ *millions*	(%)
Animal:			
Meat (inc. cattle, sheep, offal and horse meat)	8800		
Dairy produce (inc. produce cattle, buffalo, sheep and goats)	8860		
Wool, hides, tallow	1450		
TOTAL (grass-eating animals)	——	19,110	38·0
Grains:			
Rice	3784		
Wheat	3536		
Maize	3204		
Others	4430		
TOTAL (grains)	——	14,954	29·8
Potatoes, yams, cassava, etc.	6488	6488	12·9
Plantation crops:			
Sugar (beet and cane)	1307		
Cotton	1680		
Tea, coffee and cocoa	1566		
Tobacco	1225		
Rubber	458		
Oilseeds	3150		
Vegetable fibres	295		
TOTAL ('Plantation crops')	——	9681	19·3
WORLD TOTAL* (selected crops)		50,233	100·0

* Omits all horticulture and viticulture crops other than those listed in table.

This is true in most countries and I know of cogent examples (Africa) where the coffee crop drew (in 1951) the interest of a score of first-rate research minds, while the problems of grassland husbandry occupied the attention of only one graduate research scientist. At the same time, within the same territory, animal products from grassland had an annual export value 3½ times that of the coffee crop. This indicates a lack of appreciation of the value of grassland in relation to other crops, and this lack seems to be world wide. It is particularly to be deplored in so-called underdeveloped countries where, characteristically, the pastoral acreage accounts for a large part of the total land surface within a particular country. (C) Where grassland problems have received much less than their fair share of scientific attention it seems to me that the need to rectify is of some urgency in a world where medical opinion is emphatic that a large part of the human population is underfed and in particular lacks the high quality animal proteins.

Let us look a little closer at the five major pastoral groups, but before doing so I must refer to Table 5, which defines 'cattle units', and to Table 6, which shows that almost half of these are maintained in the tropics (as defined). Table 7 shows the acreage occupied by each of the five pastoral groups and the number of 'cattle units' maintained on each.[1] Total 'cattle unit' numbers are 871 million; it is of interest to record that this figure (based on my calculations) is in close agreement with the figure of 870 million calculated from data by F.A.O.[6]; the latter is based on national statistical returns (but often excluding U.S.S.R.).

TABLE 5

DEFINITION OF 'CATTLE UNIT'*

(*based on data from Barker* et al.[1])

One '*cattle unit*' = 1000 lb. cattle beast in average store condition and has its equivalent in the following:

Total cattle units = ⅔ total cattle (all age groups)
= ⅔ total buffalo (all age groups)
= × 6 total sheep (all age groups)
= × 8 total goats (all age groups)
= ⅔ total horses, camel, asses, mules (all age groups)

* Domestic animals only. No account taken of wild herbivores, e.g. rabbits, wildebeest, antelope, guanaco, deer, etc., whose numbers are not known.

TABLE 6

'CATTLE UNITS' IN
(1) 'TROPICS' TO 30° N. AND S. OF EQUATOR,
(2) THE REMAINDER (OMITS U.S.S.R.)

(millions of 'cattle units')

	1 *'Tropics'*	2 *Remainder*	*Total*
Total	427·2	442·6	869·8
Percentage	49·1	50·9	100

TABLE 7

WORLD GRASSLANDS: ACREAGES AND 'CATTLE UNITS' SUSTAINED ON PRODUCTION GROUPS 1–5

Pastoral group	1 *Very intensive*	2 *Intensive*	3 *Moderately intensive*	4 *Extensive*	5 *Very extensive*	*Total*
Total acres (millions)	93	702	1080	2052	1902	5829
Percentage	1·6	12·0	18·6	35·2	32·6	100
'Cattle units' per 100 acres of grassland	80	40	20	10	5	
Total 'cattle units' (millions)	74	281	216	205	95	871*
Percentage	8·5	32·5	24·5	23·6	10·9	100
$\frac{\%\text{ cattle units}}{\%\text{ area}} \times 100$	530	222	132	66	33	

* F.A.O.[6] data suggest a figure of 870 millions. The close agreement is significant particularly since my figure of 871 millions is based on an entirely different approach.

I wish here to record my personal debt of gratitude to F.A.O. for the international agricultural statistics which they have made available during recent years. Figures of comparable accuracy were not available previously; indeed without access to F.A.O. publications, the present analysis of world grassland would have been quite impossible. Table 8 summarises the total meat and dairy production on a global basis, divided as between tropics and extra-tropical areas (but excluding U.S.S.R. for which I have no data). The tropics produce one-third of the total meat, one-fifth of the milk and butter (including ghee) and one-seventh of the cheese. Let me remind you again that the tropics carry about half the total 'cattle units' (global basis).

TABLE 8

MEAT,* MILK, BUTTER AND CHEESE PRODUCTION IN

(1) 'TROPICS' (30° N. AND S. OF EQUATOR)
(2) REMAINDER (EXCL. U.S.S.R.—NO DATA)

('000 metric tons)

	'Tropics'	*Remainder (excl. U.S.S.R.)*	*Total (excl. U.S.S.R.)*
Meat*	9224	16,906	26,130
Percentage	*35·3*	*64·7*	*100*
Milk	55,196	198,004	253,000
Percentage	*21·7*	*78·3*	*100*
Butter	838	3128	3966
Percentage	*21·0*	*79·0*	*100*
Cheese	409	2515	2924
Percentage	*14·0*	*86·0*	*100*

* Beef, veal, mutton and lamb only.

Table 9 is of interest; it shows the average annual yields of milk per cow in five different countries, each of which represents pastoral groups 1–5. Within the existing nutritional environment of Group 5 it requires 20 milking cows to produce the same amount of milk as is given by one cow in the nutritional environment of Group 1. I have purposely used the term 'nutritional environment' in this context because there are numerous examples of dairy herds geographically situated in pastoral Groups 5, 4 and 3 which are fed chiefly on grass but at nutritional levels common to dairy herds in Groups 1 and 2. Such herds produce milk in the sort of annual quantities considered normal in 'advanced' grassland communities (pastoral Groups 1 and 2). The basic reasons for the low average yield (Groups 3, 4 and 5) I am convinced, are nutritional, rather than genetic or climatic. Since grass must be the basic food for these animals, the problem of the improvement in animal yields is basically a problem of grassland production and of improvement of quality on the grazings.

There is immense scope for research into the combined problems of grassland and the grazing animal throughout the world. Without this there seems no likelihood of any material increase in the output of animal products. Over vast areas of the globe there is evidence to show that the production from grass-eating animals is restricted because at present the livestock numbers are too large for the available food supplies on these grasslands. Until recently most of the investigational work relating to grassland has been done within Groups 1 and 2, and very little of this can be applied directly to problems of the extensive pastoral belts. However, active research is now in operation, particularly in Queensland,[4] Kenya[2] and Jamaica.[8] There is urgent need to extend the number of grassland research centres and the facilities offered to each. From a global viewpoint it is imperative that such centres should be strategically placed (irrespective of national frontiers) so as to deal as adequately and as quickly as possible with the vast pastoral areas which they would be designed to serve.

I have dealt so far with the *status quo*; let us look at potentials and at what evidence exists to suggest improvements in the productivity of grassland and of animal product. I begin by considering the potential (within existing knowledge) of Group 2, which seems to be about double the present 80 'cattle units' per 100 acres (i.e. twice the present carrying capacity). This level of production is already achieved on the best grassland farms of Britain.

The major observable difference (in the United Kingdom at least) between Groups 1 and 2 is the level of human initiative and technical ability as applied to grassland husbandry and bears little or no relation to differences in soil or local climate. It seems therefore that given similar farming techniques, Group 2 has a potential similar to that

of Group 1 (Davies[5]). A great deal of investigational work has been already done, but more critical research is still needed if yet higher yields are to be attained in both these groups.

Let us turn now to Group 3. The special need in this case is for experimentation, proceeding collaterally with basic research into local grassland problems. The 'Mediterranean climate' countries the world over fall into this group. Excellent research work in grassland has been done at the Waite Institute and other centres in Australia, the findings of which might find some application throughout Mediterranean Europe. Australian research findings have contributed enormously to indicative evidence from a number of different situations, notably that from Australia[4] which suggests that techniques for pasture improvement are available which could lead to upwards of a four-fold increase in output of animal product. Other researches in Kenya,[2] Southern Rhodesia[10] and Brazil[7] point to similar conclusions. The requirement is to extend this type of research and to ensure it is well planned and carried out by competent people.

As regards Group 5 (very extensive pastoral), the studies in Australia, North America and throughout Africa suggest that great improvements follow the knowledgeable use of the fire stick,

TABLE 9

ANNUAL YIELD OF MILK PER COW IN PASTORAL GROUPS 1–5 —SELECTED EXAMPLES FROM F.A.O. (1956) RECORDS

	1	2	3	4	5
Pastoral group	*Very intensive*	*Intensive*	*Moderately intensive*	*Extensive*	*Very extensive*
Yield per cow (kg.)	3900	2900	1460	540	190
(gal.)	860	640	320	119	42
Relative	100	75	37	14	5

increased output from Australian pasture lands. One sheep to the acre has been replaced by four or more sheep to the acre, as a result of linking sub.-clover and phosphates. In some situations certain trace element deficiencies occur and can be corrected by appropriate fertiliser treatment. Evidence concerning Group 3 suggests that the potential offers a four-fold increase in the output of meat, milk, wool, etc., from a wide range of situations.

The pasture and animal nutrition advisory services in leading grassland countries are much concerned with uplift in productivity which suggests that technological evidence, already in being, is not yet applied in practice. This is perfectly true, but it must not be taken as an excuse for any relaxation of the research effort, because further advance will depend upon the amount and quality of the research accomplished.

In regard to Group 4 (extensive pastoral) there is again paucity of research evidence, and it is not easy even to guess the potentials accurately until more research is done. There is, however, some paddocking, erection of fences, the provision of water for cattle and sheep. I have said little about irrigation, which can obviously offer one answer, but it needs to be made clear that irrigation can influence only a small part of the huge area concerned. It is possible to place over-emphasis on irrigation. My view is that the main technical effort should be put into the problems of dry land farming, which in most instances represents the major problem of pastoral areas. One should irrigate where water is available, but there can be no fundamental difference between the problems of irrigated grasslands in an otherwise arid situation and the problems of grassland in general.

A striking feature of Groups 4 and 5 is the serious lack of agronomic evidence. An integrated programme of grassland research is a prime requirement in connection with these lands throughout the world. In general, they carry more stock than the present vegetation can possibly maintain, and vast areas are in active retrogression. Carrying capacity is decreasing quite rapidly (e.g. in Patagonia and many other situations in every

continent). There is frequently serious soil erosion causing widespread denudation, examples of which can be found in India, America, Africa and even in Europe.

I want to emphasise the theme of research, soundly planned and purposefully applied to the problems of world grassland. It is only in comparatively recent times that the scientist has become interested in grassland and has applied scientific method to its study. The grasslander therefore is lagging behind his scientific colleagues in crop husbandry. This is particularly true as concerns problems of the vast pastoral areas. Together these occupy nearly 5000 million acres which accounts for 86·4 per cent of world grassland, and there is an awful lot we do not know about the way to use, let alone develop these. The scientist and especially the grassland/animal agronomist must tackle the problem, for without detailed attention from the agronomist I see no future for these lands other than degradation, soil erosion and ultimate denudation. The problem seems urgent, and I can only hope that the attention of people, and indeed of governments, everywhere will quickly become focused on this matter as an essential part of the problem of feeding a rapidly increasing human population.

I have emphasised the need for research, but to conduct purposeful research in grassland agronomy there is need not only for capital and equipment but for trained scientific personnel. To fulfil the latter requires the systematic training of research agronomists and of skilled technicians who can carry the findings of research to the farm, estancia and cattle station. Capital, equipment and other facilities are often not difficult to obtain for when governments are convinced the money will usually be found. I am more concerned with the fact that the scientific personnel is not readily available, for too few are trained to tackle these problems. I suggest, therefore, that such centres as Brisbane, Kitale, Jamaica and St. Croix (West Indies), Dr. Grossman's station in Southern Brazil and others should be asked to offer adequate training for grassland personnel. I feel assured that research centres generally would freely co-operate if the problem was put to them. I would without hesitation commit Hurley to do its part. We should be very happy to train people in the fundamentals of grassland research; indeed, we have already trained a number and these are doing excellent work in several parts of the world.

A word in brief about some of the problems of British grassland. Let us not think that all underdeveloped land is overseas. Our own grasslands in this country are grossly underdeveloped, not only in the hills but also in our lowlands. Our grassland research activities have been concentrated in the lowlands (below 1000 ft. contour). Politically and scientifically we have paid too little attention to our hills, although they include 46 per cent of all grassland in the United Kingdom.[3] The pioneer work initiated by Sir George Stapledon at Aberystwyth has had some effect in Wales and important influences elsewhere, but not nearly enough has yet been accomplished. To some degree the politician, administrator and even the agricultural scientist has shown a lack of enthusiasm in connection with the improvement of British hill land. This 'tread warily' attitude must inevitably indicate that as a nation we have no enthusiasm for the economic development of the hill lands, and their potential is not being recognised. The fact is that we have within our own shores this large, completely underdeveloped countryside—17 million acres of it—lying chiefly above the 800 ft. contour. Some of this land lies steep and the slopes may be boulder-strewn, providing a mecca for the tourist and for the poet. There is a very good case for developing this rugged country for tourism.[9] This is the hill topography which is impressed on the public mind as being representative of the mountains of Britain. Unfortunately also it is the picture dominant in the mind of those in authority who may have only passing knowledge of our hill country.

I want to emphasise most strongly that the problem of hill improvement and the development of grassland potential in the British uplands does not primarily concern the rugged and rocky screes but much concerns the huge upland plateaux and the relatively sheltered elevated valleys which intersect them. These plateaux are extensive in Scotland as well as in the North of England and throughout Wales. In total land area they greatly exceed that of the steep, intractable slopes. In a reconnaissance survey made some years ago (not yet published) I found that upwards of three-fourths of Wales (above 1000 ft.) is represented by undulating land which has much the topography

TABLE 10

EWE POPULATIONS IN SELECTED 'HILLY' COUNTIES IN ENGLAND, WALES AND SCOTLAND

	Rough grazings ('000 acres)	*Percentage of total agricultural area*	*Breeding ewes* ('000)	*Acres of rough grazings* (per ewe)
England and Wales:				
Four 'hilly' counties*	907	60	761	1·2
Scotland:				
Four 'hilly' counties†	5832	94	695	8·4
TOTAL (8 counties)	6739	88	1456	5·2

* Brecon, Merioneth, Radnor and Westmorland. † Argyll, Inverness, Ross and Cromarty, and Sutherland.

Notes

1. England/Wales hills carry about one breeding ewe per 3 acres, but when integrated with valley intakes (as in most situations) the overall carrying is one breeding ewe per 1·2 acres.
2. In Scotland the 'hills' are more self-contained as well as more rugged. The overall carrying is about one ewe to 10 acres, but with some integration with valley lands this is averaged at one breeding ewe per 8·4 acres.
3. Average for all 8 counties (where Rough Grazings = 88 per cent of Total Agricultural Area) is one ewe per 5·2 acres. This tallies well with total of 4 million hill ewes on 17 million acres of Rough Grazings = one breeding ewe per 4¼ acres.
4. This tallies with Table 4, where Rough Grazings in United Kingdom are classed as Pastoral Group 4 ('extensive' = 10 cattle units per 100 acres).

of farmlands throughout the Midlands of England. The same is true of the elevated country elsewhere throughout the British Isles, although the proportion of rugged slopes is somewhat higher in parts of Western Scotland. If my estimate proves reasonably accurate, then upwards of 10 million acres of hill land lie on easy undulating slopes and are consequently tractable. There are abundant examples to indicate that development is well worth while within a grassland farming-cum-forestry system. There has been much heated discussion as to the place of the forester on the hills of Wales and of Scotland. There is clearly a place for both forest and grassland and for one I would be very happy to see the steep escarpments and boulder-strewn lands under forest. Surely, it seems sensible to plan in such a way that forest, grass and livestock farming in the hills shall be integrated and to do so with a view to make the most of our countryside and of its land potential. To achieve a sensible and practical plan of development demands co-operative research between those interested in forestry and in grassland improvement. Unfortunately during the past quarter-century these various interests have shown little evidence of co-operative development—the attitude of both sides has savoured too much of 'dog and manger' and this has got us nowhere. For the sake of the hills and perhaps for the sake of British agriculture as a whole I sincerely hope that the present attitudes will change and that this will lead to a better appreciation of hill potentials which will surely be developed as a consequence (Table 10).

I hope the evidence I have put before you will at least have engendered the conviction that the doctrines of Malthus need not dominate the thoughts of mankind for a long time to come. The grasslands of the world offer an important field of study but this must have a basic scientific approach. The problems associated with it are manifold and it is my sincere belief that peoples everywhere must be encouraged to tackle these as a matter of some urgency. While I may have left no doubt in your minds as regards potential food supplies from the grasslands of the future, I hope that I have also been successful in impressing upon your minds the serious losses of soil which take

place every year and which are a direct consequence of the misuse of grazing lands. Soil erosion (sheet and gully) is widespread and in particular throughout the tropics which, as I have tried to indicate, contain one-half of the total world grasslands.

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ECHOES from the Past

Paradoxes and Horrible Conclusions

'My experience of the British Association does not extend quite over a geological epoch, but it does go back rather longer than I care to think about; and when I first knew the British Association, the *locus* of disturbance in it was the Geological Section. All sorts of terrible things about the antiquity of the earth, and I know not what else, were being said there, which gave rise to terrible apprehensions. The whole world, it was thought, was coming to an end, just as I have no doubt that, if there were any human inhabitants of Antrim in the middle of the tertiary epoch, when those great lava streams burst out, they would not have had the smallest question that the whole universe was going to pieces. Well, the universe has not gone to pieces. . . . At present the *locus* of disturbance is to be found in the Biological Section, and more particularly in the anthropological department of that Section. History repeats itself, and precisely the same apprehensions which were expressed by the aborigines of the Geological Section, in long far back time, are at present expressed by those who attend our deliberations. The world is coming to an end, the basis of morality is being shaken, and I don't know what is not to happen if certain conclusions which appear probable are to be verified. Well, now, whoever may be here thirty years hence—I certainly shall not be—but, depend upon it, whoever may be speaking at the meeting of this department of the British Association thirty years hence will find, exactly as the members of the Geological Section have found, on looking back thirty years, that the very paradoxes and horrible conclusions, things that are now thought to be going to shake the foundations of the world, will by that time have become parts of every-day knowledge and will be taught in our schools, as accepted truth, and nobody will be one whit the worse.'

Address by Prof. T. H. Huxley, British Association Meeting, Dublin, 1878.

* * *

Statistical Science

'I have looked through the papers which since that time (1856) have been communicated to us, and I have been struck by the unscientific character of many of them.

'I use that word not dyslogistically but merely distinctively, merely as expressing that the writers had wandered from the domain of science into that of art.

'I need scarcely remind you that a Science is a statement of existing facts, an Art a statement of the means by which future facts may be brought about or influenced. A science deals in premises, an Art in conclusions. A Science aims only at supplying materials for the memory and the judgment. It does not presuppose any purpose beyond the acquisition of knowledge. An Art is intended to influence the will: it presupposes some object to be attained, and it points out the easiest, the safest, or the most effectual conduct for that purpose. . . .'

'The science of statistics is far wider as to its subject matter. It applies to all phenomena which can be counted and recorded. It deals equally with matter and with mind. Perhaps the most remarkable results of the statistician's labours are those which show that the human will obeys laws nearly as certain as those which regulate matter. . . .

'. . . (The statistician's) task is over when he has stated and recorded (the facts). It is the business of the legislator to draw from the figures of the statistician, practical inferences. To ascertain the

circumstances, moral, commercial, or political, under which the tribute paid by his countrymen to insolvency, crime, sickness and death, has been increased, has been diminished, or has remained stationary—these circumstances will often appear to be under control, and by watching the statistical results of every attempt to control them, he will ascertain whether they are under control or not.

'We have been told that a statesman "reads his history in a nation's eyes". I should rather say that he reads it in a nation's figures. . . .'

Address by Nassau Senior, B.A. Meeting, Glasgow, 1860.

* * *

Sydney Smith on Science

To The Editor of 'The Times'

SIR,—In connexion with the meetings of the British Association at Blackpool, it is interesting to read what Sydney Smith thought of the meetings of the British Association at Bristol a hundred years ago. Writing to a friend in the summer of 1836, he observed:

> I thought you had been paying a visit to the wise men of Bristol. Their occupations appear to be of the greatest importance. One Monday they dissected a frog; on Tuesday they galvanized a goose; on Wednesday they dissected a little pig, and showed that wonderful arrangement by which that animal is enabled to squeak or grunt at pleasure. On Thursday they tried to go up in a balloon; but the balloon would not stir. The causes of the failure were, however, pointed out in a most satisfactory manner. On Friday a new and philosophical mode of making butter was brought forward, and several pats of butter exhibited. On Saturday they all set off on the outside of coaches, believing they had conferred the utmost benefits on the human race. You and I know better.

Yours faithfully,
HESKETH PEARSON

St. John's Wood. *'The Times', Sept.* 14, 1936

* * *

Joule

Speaking (at the Blackpool Meeting in 1936) of the nineteenth-century physicists, Prof. Ferguson said: 'It is an odd fact that these days of probability and indeterminacy mark a period in which atomic and molecular constants have been evaluated to a degree of accuracy of which electrical standards need hardly be ashamed. And we may perhaps be pardoned a little local patriotism when we remember that a Manchester man, James Prescott Joule, made the first determination of an absolute molecular magnitude—the mean speed of a hydrogen molecule, which he evaluated as 6,055 ft. per second at the freezing-point of water. This paper, which was published in 1848, is not the paper which was denied publication in extenso by the Royal Society, concerning whose refusal Joule remarked to Schuster, "I was not surprised. I could imagine these gentlemen in London sitting round a table and saying to each other: 'What good can come out of a town where they dine in the middle of the day?'".'

Address by Prof. A. Ferguson, British Association Meeting at Blackpool, 1936.

* * *

The Scientist and Society

'Public life, not less than science, is a jealous mistress, and does not well tolerate a known devotion to any other pursuit. It has besides a science of its own, essential to it, especially in a free country,—the knowledge of man; and this is not always the special gift of men of science, who deal less with men than with things and thoughts; and I am not sure that the qualities, which fit a man for success in the one pursuit, are peculiarly advantageous to him in the other. This, however, is certain,—that those who administer the affairs of this country ought at least (I do not think as yet they do) to know enough of science to appreciate its value, and to be acquainted with its wants and with its bearings on the interests of society; but such knowledge, I cannot doubt, will soon become the common appanage of all well-educated men; and when it is so, as I said before, whatever, either in the position of science itself, or men of science, is still wanting, will soon be supplied.'

Presidental Address by the Earl of Harrowby, Liverpool, 1854.

LOOKING AT science

A SURVEY OF THE USE OF VISUAL AIDS FOR SCIENCE IN SECONDARY SCHOOLS IN ONE LOCAL EDUCATION AUTHORITY

Whilst there is an abundant supply of visual aids for the presentation of science there is not a great deal of available information about their use. The Visual Aids Committee of the British Association, therefore, requested the Visual Aids Officer to make a survey of the use of visual aids in the presentation of science in Secondary Schools in one Authority, so that the Committee would have more factual information at its disposal. A large Local Education Authority very readily co-operated, and the Visual Aids Officer was given access to whatever departments and schools he wished to visit.

The folowing report was presented to the Visual Aids Committee and it was agreed to publish an appropriate version of this with extensive verbatim quotations from interviews. This publication will be available in the autumn.

Scope of Survey

Fifty-one of approximately 200 Secondary Schools in the selected area were visited. These were selected to give as representative a picture as possible of different types of schools in different areas. The Authority is divided into eleven divisions, and schools were visited in seven of these. They ranged in size from one with 240 pupils (including 30 residents) to another with 1150 day students.

TYPES OF SCHOOLS VISITED

	Boys	*Girls*	*Mixed*	*Total*
Grammar	5	5	4	14
Technical	–	–	6	6
Secondary Modern	4	5	15	24
S.M./Technical	–	–	3	3
S.M./Grammar	–	–	2	2
Technical/Grammar	1	–	1	2
	10	10	31	51

Informal interviews were held with 26 headmasters, 7 headmistresses, 66 men science teachers, 14 women science teachers, 23 men art teachers and 5 women art teachers. Informal discussions were held with one third-year Secondary Modern class in a Bilateral School, and with girls from several forms at the inaugural meeting of a science club in a Girls' Grammar School.

This particular authority was selected for a variety of reasons—it has actively promoted the use of visual aids since 1940; it covers a large area embracing erstwhile independent Borough Education Authorities; old-established industrial areas; comparatively new industrial areas; remote rural areas; old country towns; and two new towns.

As part of its Visual and Aural Aids Service the Authority provides its Secondary Schools with both 16 mm. sound projectors and filmstrip projectors (and is in fact just completing the changeover from silent to sound projectors in all primary schools). These and other pieces of equipment (tape recorders, record players, rear projection screens, etc.) are all of standardised design, to reduce initial cost and facilitate maintenance. Whilst every school visited had this basic equipment many had additional equipment acquired in various circumstances. Thus three schools had two film projectors each, one boasted five filmstrip projectors, and another had two micro-projectors. The Authority provides an exceptionally cheap service for teachers to make their own slides and filmstrips; organises courses on the use of equipment and on the making of filmstrips and films; provides a mobile audio-visual service for some schools; and has a Film Library which contains 3500 copies and distributed 24,000 films in 1959. Several of the divisions provide Filmstrip Libraries as an additional service.

Restrictions on the Use of Visual Aids

The use of visual aids is conditioned by many factors however, and the survey suggested a far more limited use of visual material than the provision of the above facilities would suggest. Restrictions on the use of visual aids included:

(*a*) *Blackout*. This is a major problem, more particularly with new buildings with generous window space, e.g. one school opened 18 months had not yet used its projection equipment as it had no blackout at all. The existing provision of blackout is often inadequate (some transparent blinds and curtains; old, sometimes rotting, blackout material; failure to screen the surrounds of windows). One school was just replacing a blackout system based on blinds fitted outside the windows:

(*b*) *Ventilation*. Frequently lack of proper ventilation in blackout conditions restricts the use of visual aids. This appears to be as much a problem in new buildings as old.

(*c*) *The use of equipment*. Weight and size often impede their use, e.g. the Authority's standard 16 mm. sound projector mounted with its transfomer on a trolley discourages mobility in multistorey buildings; electric points are often absent or inconveniently located; one school until recently had both A.C. and D.C. in different parts of the building; some had different sized power points in different rooms; fixed screens in several schools were badly located; lack of display space precludes the use of charts; one school had seven classes in outside accommodation up to two miles from the central building.

Many teachers are reluctant to use equipment either through lack of training or because they do not feel the trouble is justified. Staff turnover further complicates this problem in some schools.

(*d*) *Finance*. Schools have an allowance per student from the Local Education Authority for the purchase of equipment ('everything, including broken windows'). Expenditure on visual aids from this capitation allowance tends to have a very low priority. This is particularly the case with film hire and almost every school visited concentrated on free sources.

The Use of Visual Aids and Attitudes towards Them

(*a*) A minority of teachers was found who used no prepared visual aids whatever.

(*b*) Films and filmstrips were the most popular aids and opinions were evenly divided about their relative value. Hardly any used slides, and charts (rarely used for teaching) were mainly used for indirect information to children and occasionally (according to some teachers) purely for decoration. Models (principally biological) were used in a number of schools but construction kits were never seen and very few teachers expressed interest in them.

(*c*) In Grammar Schools there was a marked tendency for the use of films for science teaching to be confined to the lower forms—the argument used being that by the time students reached Fifth and Sixth Forms, they not only had to keep abreast of a crowded syllabus but were also competent to learn what was required of them from reading and practical work. One notable exception however was a Grammar School which had used up to 200 films in a year for teaching (mainly science) and claimed that it was in the lower forms with only two or three periods of science a week where there was no time for films; and they showed films mainly to the Fifth and Sixth Forms.

(*d*) A minority of teachers interviewed were enthusiastic about visual aids, but the general impression was that they played little part in the thinking of the average teacher when planning work and teaching his subject. It is significant in view of staff shortages and difficulties (which were complained of in many schools) that no teacher interviewed subscribed to the view that films could be an effective substitute for the teacher in these circumstances.

(*e*) The number of films and filmstrips used in school is very limited, even when members of the staff are knowledgeable about them and interested in their use. An extreme instance was one Secondary Modern School, where the science master not only had a wide knowledge of the medium but possessed a ciné-camera, projector and tape recorder of his own. Yet he used only three films a year for teaching. However it should be noted that a film may get several screenings (one school visited showed one film to fourteen different classes in one week.)

(*f*) Eleven schools had Film Societies and there were others which included film shows in the activities of science clubs and other school societies.

(*g*) It was noticeable that the most enterprising use of visual aids was in the Secondary Modern Schools.

(*h*) Four schools had ciné-cameras and made films and a fifth was about to start. In all cases the films made were either records of school activities or entertainment films. In no case had films been made on scientific subjects.

(*i*) Eight of the schools visited had television. It was treated with considerable reserve as a teaching aid and the two chief complaints were the difficulty of fitting programmes into a timetable and the ephemeral nature of the programmes themselves. (Timetable difficulties are common with sound broadcasting but schools overcome them by making tape recordings. Several teachers thought that the television authorities should make telerecordings of science programmes available to schools.)

General Conclusions

(*a*) Science is often taught in the face of considerable difficulties and it is apparent that the biggest contribution to a wider understanding of science and its technical and social implications would be by an improvement in staffing,* by more generous provision of equipment and facilities in schools, and by closer liaison between all types of schools and industry.

(*b*) It was most noticeable how teachers (particularly Head Teachers) welcomed the opportunity of unburdening themselves about their problems. They naturally discussed visual aids but often spontaneously raised many other subjects including problems of staffing; the training of teachers; school architecture; the place of science in the curriculum; and the conflicting demands of industry and society.

(*c*) It was apparent that whilst a minority of the teachers interviewed were keen on visual aids, their use played no major part in either the teacher's planning of his lessons or their presentation. There was little indirect evidence to show that training colleges generally placed much emphasis on visual aids. One had the impression that much more needs to be done to show the teacher ways of using visual material in teaching his specialist subject, and ensuring that appropriate visual material is available for this.

(*d*) Lack of information about sources of visual material was common (including ignorance about the County Visual Aids Service). One was impressed with the fact that even the keen user of visual aids quickly settles into a pattern of presentation and is, for example, often ignorant of suitable films which are listed in the catalogues he uses.

(*e*) Teaching to a syllabus may inhibit experiment, and it was the schools without examination candidates which showed the most novel approach to the use of visual material.

(*f*) None of the schools which made films had produced anything concerned with science, yet one small Secondary Modern School has won British Film Institute awards with its feature films, which include some excellent animation in both black and white and colour. One girls' Secondary Modern School had made a biology film strip.

Recommendations

(*a*) Because schools are so limited in the money they can spend a big increase in the distribution of visual material presupposes an increase in the supply of its material. The use of appropriate visual material is an essential part of the presentation of science to young people. To ensure the success of its Young People's Programme, therefore, the British Association should do everything in its power to obtain adequate production and distribution of appropriate visual aids.

(*b*) The biggest immediate need is for more visual material related either directly to syllabus teaching (in the case of schools whose students take public examinations), or to well-defined patterns of teaching which fit in with the vocational and social needs of the Modern School student. At the moment most of the limited science teaching material which is available is designed for advanced groups and there is obviously a big need for much more material designed for the lower forms of Secondary Schools.

(*c*) There was some evidence that science teachers experienced difficulties arising from either the absence of science teaching in Primary Schools or the methods of presenting science where it was taught in such schools. There is a case for a survey of existing work in science teaching in Primary Schools in the area now surveyed, to supplement

* The Science Masters' Association Secondary Modern School Committee was reported in *The Advancement of Science* of July, 1960, as stating that at least 2500 schools need science teachers.

the information obtained from Secondary Schools and provide more guidance about the provision and co-ordination of visual material at all stages in science education.

(*d*) In general teachers appeared unaware of the value of visual material (particularly films) as a source of information for themselves. The British Association should help here both centrally and through Area Committees in arranging viewings for teachers at locally convenient centres and in individual schools.

(*e*) Television: Because of timetable problems television is severely limited in its use in those schools which have equipment. The teachers in these schools were generally critical of the programmes as containing too much information and tending to be too general in character. Yet this type of programme would appear very suitable as a basis for science club activity and there seems a strong case for the British Association discussing with the television authorities the establishment of a Television Science Club, with which the Association should be closely associated.

(*f*) The interest of the British Association was obviously welcomed by the schools and there is a strong case for regular consultation with science teachers to plan with the Association suitable extra-curricular activities.

BOOKS

What is Cybernetics? By G. T. Guilbaud. (Heinemann, London.) 10*s*. 6*d*.

This book gives an impeccable translation from the French of a popular short essay, written for the 'intelligent layman', by the Director of Studies of L'École Practique des Hautes Études, Paris.

What is Cybernetics? The book opens wisely with a few historical notes, reminding us in particular of Jacques Lafitte's remark that (in 1932) 'the science of machines does not exist'. Machines . . . the 'organised constructions of man' . . . artefacts; what are their limits?—what can or cannot they do? —learn? — adapt? — predict? — show intelligent and purposeful behaviour?—make decisions? The word 'cybernetics' is commonly used today to signify this field of interest and including attempts to draw general conclusions, by analogy with other 'organised systems'—even biological. Such attempts at unification are characteristic of our time; synthesis, above all, is sought.

Lady 'Cybernetics' came of age some ten years ago, a modest maid; unfortunately, so loudly have her charms and gifts been sung by some extravagant writers, that she is now finding herself a little shunned in respectable company. But M. Guilbaud takes her seriously and shows us the genuine qualities of her character, in simple words.

The book opens with a comparison of different but familiar *control systems*; the Watt governor, the thermostat, the motor car . . . illustrating the economy provided by the '*closed loop*' principle (it has no advantage other than economy), giving automatic self-setting to the device. The broad analogy with all *goal-directed* activity is explained; *reflex* actions such as exist in the nervous system, in economic systems and in all *systems* which maintain themselves in *regulated stability*. All such systems contain a *source* which is *controlled* by the flow of *information*.

The various chapters explain and illustrate with homely examples the various terms I have italicised here. In particular the concept of *information* is examined scientifically (as in Information Theory) together with the related concepts of *chance*, *choice* and *probability* as arise in any intelligent, purposeful, action.

This book is not for experts; yet it may show them what solid worth may one day blossom from the compost heap of cybernetics . . . 'the bud may have a bitter taste, yet sweet will be the flower'.

COLIN CHERRY

Men and Molecules: What Chemistry is and What It Does. By Carl R. Theiler (translated from the German by E. Osers). 214 pp. (Harrap, London.) 21*s.* net.

Because of its dependence on concepts not made familiar by everyday experience, and more particularly because of its dependence on the use of formulae, the popular exposition of chemistry presents exceptional problems. While this book does not wholly overcome these inherent difficulties, it is certainly a creditable attempt at giving the non-technical reader a picture of the modern chemical industry and the chemistry on which it is based. While the pedant may complain of over-simplification, it is inevitable that some sacrifices must be made if the story is to be readable and interesting. If a book of this kind achieves these two aims it does well, for the reader may be encouraged to seek fuller information elsewhere; if he finds even a popular account difficult and tedious he is scarcely likely to want to learn more.

For its purpose this is quite a well-balanced prescription. For British readers, a useful feature of the book is that it lays more emphasis on the Continental chemical industry than most books available in the English language. While the author goes astray at some points—for example, in respect of some details in connection with the development of penicillin—and there are a few unexpected omissions—for example, of Castner's process of aluminium manufacture—the text seems free of major errors.

TREVOR I. WILLIAMS

Selected Lectures in Modern Physics for School Science Teachers. Edited by Prof. H. Messel, University of Sydney. (London, Macmillan and Co. Ltd., 1960). VII + 328 pp. 30*s.* net.

From time to time every senior teacher of physics in a university or in a technological institute must experience a demand or a desire to provide a course of lectures in modern physics which should interest those who have only a basic knowledge of the subject. He will be lucky indeed if in arranging such a course he can count upon the assistance of nearly twenty lecturers, some of them of world repute.

Now, Prof. Messel has had the good fortune to have such assistance in providing a series of some twenty-two lectures as a refresher course for science teachers at a Summer School in the State of New South Wales, and the lectures are now available to us in a book, of which, incidentally, every science teacher in New South Wales received a free copy. From them an intelligent reader may obtain a good grasp of the fundamentals of nuclear processes, and he will certainly appreciate Oliphant's lecture on nuclei in collision, and Schafroth's lecture on strange particles with its references to strangeness and to parity.

Cosmic rays and their importance in ultra high-energy nuclear physics form the subjects of three well-illustrated lectures. Solar activity, radio astronomy and the physics of the high atmosphere form the subjects of three others. Space travel and problems of relativity find a place; so also do descriptions of the properties of superfluids and of transistors, and of the uses of isotopes in medicine. In addition, there are interesting lectures on precise measurements and on the concept of temperature.

Some suggestions are made for the provision of simple laboratory exercises in modern physics, and these suggestions might well be expanded in a future course. It is perhaps worth while to mention that in almost every senior physical laboratory in the London technical colleges are to be found experiments on proton resonance, while magnetism in this course is mainly confined to rocks and to stellar regions. But, the compendium is a very useful one, and the reviewer is sure that many British teachers of physics will find its perusal rewarding.

L. F. BATES

Nature and Man. By John Hillaby. Progress of Science Series, edited by Nigel Calder. 64 pp. (Phoenix House, London.) 9*s.* 6*d.*

This is the ninth book in a series which is written 'specially for young people exploring the very boundaries of the world of scientific development'. If my own experience is anything to go by, it can be read with profit by older people. I nearly wrote 'read with enjoyment' but Hillaby's factual account of the accelerating rate of the destruction of animal and plant life practically all over the world will probably evoke gloom and perhaps a feeling of guilt, for all of us are to a greater or lesser degree responsible for indiscriminate killing of wild life.

If some of the writing could be classed as journalism, in the sense that it highlights the dramatic

aspects of ecology, it is always very good journalism and Hillaby can never be accused of being prolix or ambiguous. Moreover, making slight concessions for popularisation, the book is very accurate. It begins by a brief statement of the human over-population problem and then goes on to draw parallels with population pressures in animal communities. There is a good account of the inter-relations between living organisms, of animal migrations and of the dangers attendant on the introduction of species to novel surroundings. In succeeding chapters Hillaby, by referring to well-documented examples, makes it abundantly clear that the main factor disturbing the 'balance of nature' is the activity of Man, whether by avarice, 'sport' or sheer ignorance. Hillaby makes a strong plea for overall conservation of our natural resources and explains the work of the various organisations which have this as their aim. His book should be welcomed by all those who would not wish the world to be populated in a century or so merely by sparrows and starlings; rats and mice, insects, nematodes, protozoans—and, of course, Man.

G. E. NEWELL

A Short History of Scientific Ideas. By Dr. Charles Singer. Pp. xviii+525. (Oxford: Clarendon Press.) 35*s*. net.

This book grew out of the late Dr. Singer's well-known work, *A Short History of Science*, which was first published in 1941. It describes the development of the natural sciences down to the beginning of the present century and deals with western science, including the Arab contribution after the fall of the Western Roman Empire, but not with science in India or China. It assumes in the reader a wide cultural background but a level of scientific attainment no higher than that of an ordinary secondary education. A commendable and unusual balance is maintained between the space devoted to biology on the one hand and to the physical and chemical sciences on the other; and Dr. Singer drew very widely on his recent experience in editing *The History of Technology* in rewriting the present work.

Historical writings on science lie broadly between two poles. There is the point of view from which the only history properly so called is that of the several sciences, to be studied in detail and to some extent in isolation, mainly with reference to the work of individual scientists. At the other extreme is the approach of the historian of ideas, who sees the sciences as threads in the cultural fabric, to be viewed as a whole among the warp and woof of ideas on philosophy, art, economics and theology with which it is interwoven. *A Short History of Scientific Ideas*, despite its comprehensiveness, falls into the former rather than the latter class, dealing with the separate sciences largely as autonomous disciplines with little more than passing reference to the social and philosophical soils in which they were nourished.

Having said this, it should be stressed that of its kind this book is in the very front rank. It seems less and less likely that any other such general work on the history of the sciences will be written by such a master of the art as Dr. Singer. The vogue for specialisation is abroad even among the historians of science and those who deal with the place of science in history (as opposed to the history of particular sciences) are liable to fall under the anathema of historicism. This comprehensive but quite elementary treatment of the development of one of the most important aspects of western culture may therefore occupy an unrivalled position among works on the subject for a long time to come. No more reliable or authoritative guide to the subject could be placed in the hands of the general reader or of the student specialising either in science or in history, to whom it may be especially recommended.

The high standard of the production of the book is that which we associate with the Clarendon Press and one of its most attractive features is the large number of excellent illustrations with which it is provided.

A. V. S. DE REUCK

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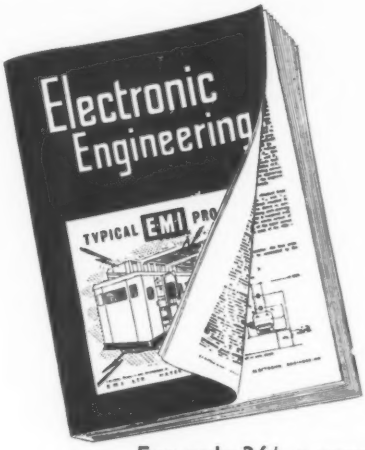

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